3D.P.90887/4

Market Penetration, Biased Selection, and Utilization in Medicare HMOs

by

Michelle Mello

A dissertation submitted to the faculty of the University of North Carolina at Chapel Hill in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the Department of Health Policy and Administration, School of Public Health.

Chapel Hill

1999

Approved by:

Sally C. Stearns
Advisor: Sally C. Stearns, Ph.D.

Edward C. Norton
Reader: Edward C. Norton, Ph.D.

Thomas C. Ricketts
Reader: Thomas C. Ricketts, Ph.D.

Steven A. Garfinkel
Reader: Steven A. Garfinkel, Ph.D.

Susan DesHarnais
Reader: Susan I. DesHarnais, Ph.D.

© 1999
Michelle Mello
ALL RIGHTS RESERVED

ABSTRACT

MICHELLE MELLO. Market Penetration, Biased Selection, and Utilization in Medicare HMOs. (Under the direction of Sally C. Stearns.)

This study investigates three research questions concerning Medicare HMOs: (1) Is favorable selection into Medicare HMOs still present in the 1990s? (2) Does the extent of favorable selection vary with the degree of Medicare HMO market penetration in a county? (3) Do Medicare HMOs significantly reduce the amount of health services used by Medicare beneficiaries relative to fee-for-service plans, once selection bias is controlled for?

Studies of Medicare HMOs in the 1980s consistently found that HMO enrollees were healthier than nonenrollees, regardless of the specific health status measure used. Despite this finding, most studies of the effect of Medicare HMOs on health services utilization failed to include strong controls for selection bias. These studies generally found health services use by HMO enrollees to be markedly lower than that of nonenrollees. The relationship between favorable selection and market penetration has not previously been submitted to empirical study.

This study uses simultaneous equation techniques, including both instrumental variables and discrete factor analyses, to examine these relationships. The data consist of a panel of linked 1992-96 data from the Medicare Beneficiary Survey, the Area Resource File, and Health Care Financing Administration reports on managed care

penetration. Identification of the system is achieved through the exclusion of market penetration variables from the utilization equations.

The study finds little evidence of persistent favorable selection into Medicare HMOs in the 1990s. While overall favorable selection has weakened over time, there is no significant indication that the extent of favorable selection varies with the extent of HMO market penetration, at least on a county level. When selection effects are controlled for, the data indicate that Medicare HMOs bring about significant reductions in both the probability of hospitalization and the number of hospital days used by those hospitalized. Medicare HMO membership slightly increases the probability that a Medicare beneficiary will use physician services. The study's results concerning favorable selection suggest that the AAPCC payment formula may no longer be significantly overpaying HCFA's risk contractors.

ACKNOWLEDGMENTS

I wish to warmly thank the members of my dissertation committee, Sally C. Stearns, Edward C. Norton, Thomas C. Ricketts, Steven A. Garfinkel, and Susan I. DesHarnais, for their thoughtful guidance of this project from its inception to its end. Each has contributed uniquely to the evolution of this dissertation.

I extend my deepest gratitude to my advisor, Sally Stearns, for four years of patient supervision, advice, and mentoring. Her unwavering support for my unusual dissertation timeline and career goals, willingness to advise me from afar, and great responsiveness in answering my emailed questions made it possible for me to complete this dissertation while attending law school in another state. In addition, I do not believe I would have been able to obtain the necessary releases for the data used in this study, understand the intricacies of the datasets, or obtain a grant for the study without her knowledge and experience.

I also want to express my appreciation to those who provided pragmatic and technical assistance to the project. Maribel Franey and Carl Hackerman of the Health Care Financing Administration were extremely helpful in gaining access to the data. Tom Ricketts generously opened up the resources of the Cecil G. Sheps Center for Health Services Research to me. Perhaps the most valuable resource there was Ann Howard, who tirelessly labored to merge together the MCBS datasets used in this study. The administrative support of the Sheps Center Business Office is also

appreciated. Finally, Tom Xu of the Department of Economics provided his FORTRAN expertise to program the discrete factor model analysis in the study.

Finally, I thank all of my friends and family who encouraged me in this endeavor, and who had confidence, when I did not, that I would succeed in it.

This research was funded by a Dissertation Grant from the Health Care Financing Administration (Project Number 30-P-90887/4-01). My doctoral studies were generously supported by a William R. Kenan Fellowship from the Graduate School of the University of North Carolina.

TABLE OF CONTENTS

LIST OF TABLES x

LIST OF FIGURES xi

Chapter

I. INTRODUCTION 1
- 1.1. HMOs as a response to the rising costs of care for elderly Americans 1
- 1.2. Purpose of the study 4
- 1.3. Format of the dissertation 8

II. BACKGROUND AND SIGNIFICANCE 9
- 2.1. Health maintenance organizations 9
- 2.2. Managed care and Medicare 16
  - 2.2.1. The Medicare risk program 16
  - 2.2.2. Effectiveness of HMOs for the Medicare population 24
- 2.3. Biased selection in managed care plans 27
  - 2.3.1. The concept of biased selection 27
  - 2.3.2. Previous studies of biased enrollment of the non-elderly in HMOs 31
  - 2.3.3. Previous studies of selection bias in Medicare HMOs 37
- 2.4. Significance of the research 46
  - 2.4.1. Policy significance of favorable selection 46
  - 2.4.2. Need for the present study 50

III. THEORY AND CONCEPTUAL MODEL 57
- 3.1. Medicare HMO choice 57
  - 3.1.1. Overview of consumer choice theory 57
  - 3.1.2. Econometric specification of the theory 62
  - 3.1.3. Applications and extensions of consumer choice theory in the present study 63
  - 3.1.4. Effect of individual characteristics on health plan choice 67
  - 3.1.5. Effect of market area characteristics on health plan choice 72
- 3.2. Health services utilization by the elderly 78
- 3.3. Conceptual model 85

IV. HYPOTHESES AND EMPIRICAL MODEL 89
- 4.1. Hypothesis 1: Favorable selection in Medicare HMOs 89
- 4.2. Hypothesis 2: Utilization in Medicare HMOs 91

4.3. Operationalization of variables 93
4.3.1. Health plan choice equation 93
4.3.2. Utilization equations 97
4.4. Justification of analytical methods 99
4.4.1. Simultaneous equations 99
4.4.2. Discrete factor model 106
4.4.3. Probit model 107
4.4.4. Two-part model 110
4.4.5. Panel data analysis 113
4.4.6. Complex survey design adjustments 117

V. DATA 121
5.1. Overview of dataset linkages 121
5.2. The Medicare Current Beneficiary Survey 122
5.2.1. Sampling scheme 122
5.2.2. Data reconciliation process and underreporting issue 123
5.2.3. Other validity issues 131
5.3. The Medicare Market Penetration File 132
5.4. The Medicare Prepaid Health Plans Monthly Report File 133
5.5. The Area Resource File 134
5.6. Key decisions made in the dataset merging process 135
5.6.1. The MCBS dataset 136
5.6.2. The county-level HMO dataset 137
5.6.3. Data cleaning 140

VI. RESULTS: MARKET PENETRATION AND FAVORABLE SELECTION 143
6.1. Descriptive statistics 143
6.2. Model diagnostics 148
6.3. Big panel dataset results 149
6.3.1. Instrumental variables estimation 152
6.3.2. Discrete factor model 155
6.4. Small panel dataset results 156
6.5. Cross-sectional dataset results 157

VII. RESULTS: EFFECT OF HMOs ON UTILIZATION 159
7.1. Big panel dataset results 159
7.1.1. Naïve model 159
7.1.2. Instrumental variables estimation 160
7.1.2.1. Model diagnostics 160
7.1.2.2. Regression results 164
7.1.2. Discrete factor model 175
7.2. Small panel dataset results 178
7.2.1. Naïve model 178
7.2.2. Instrumental variables estimation 178

7.2.1.1. Model diagnostics 178
7.2.1.2. Regression results 181
7.3. Cross-sectional dataset results 184
7.3.1. Naïve model 184
7.3.2. Instrumental variables estimation 185
7.3.2.1. Model diagnostics 185
7.3.2.2. Regression results 186
7.4. Explaining differences in results between the datasets 189

VIII. DISCUSSION 200
8.1. Market penetration and favorable selection 200
8.1.1. Recap of DFM results 200
8.1.2. Theoretical discussion 200
8.1.3. Policy implications 209
8.2. The effect of HMOs on health services utilization 212
8.2.1. Recap of DFM results 212
8.2.2. Modeling the relationship 212
8.2.3. Theoretical discussion 216
8.2.4. Policy implications 221

IX. CONCLUDING COMMENTS: STUDY LIMITATIONS AND CONTRIBUTIONS 228
9.1. Limitations of the study 228
9.2. Principal contributions of the study 234

X. APPENDIX A: PROGRAMMING NOTES 240

XI. WORKS CITED 249

LIST OF TABLES

Table 3.1: Theoretical Effect of Individual Characteristics on Health Plan Choice 70

Table 3.2: Percentage of Physicians with Managed Care Contracts, 1988-1996 74

Table 3.3: Medicare HMO Penetration and Physician Participation in 1996, by Census Region 75

Table 4.1: Operationalization of the HMO Choice Conceptual Model 93

Table 4.2: Operationalization of the Utilization Conceptual Model 98

Table 6.1: Sample Descriptive Statistics 145

Table 6.2: Probability of HMO Membership 150

Table 6.3: Predicted Probability of HMO Membership 152

Table 7.1: Norton Test Results – Big Panel Dataset 162

Table 7.2: Probability of Hospitalization 165

Table 7.3: Number of Inpatient Days 167

Table 7.4: Probability of Physician Visit 169

Table 7.5: Mean Predicted Probability of Hospitalization 171

Table 7.6: Mean Predicted Number of Inpatient Days 171

Table 7.7: Mean Predicted Probability of Physician Visit 171

Table 7.8: Norton Test Results – Small Panel Dataset 179

Table 7.9: Norton Test Results – Cross-Sectional Dataset 186

Table 7.10: Summary: Effects of Health Status on Probability of HMO Membership 190

Table 7.11: Summary: Effect of HMO Membership on Utilization 191

LIST OF FIGURES

Figure 2.1: Percent of Medicare Beneficiaries Enrolled in Risk HMOs, Historical and Projected 23

Figure 3.1: The Behavioral Model of Health Services Utilization 79

Figure 3.2: Average Number of Ambulatory Physician Visits Per Year by Age Group 83

Figure 3.3: Conceptual Model 86

Figure 4.1: Path Diagram of Endogenous Relationships 99

1. Introduction

1.1. HMOs as a response to the rising costs of care for elderly Americans

Public and Congressional concern over the solvency of the Medicare program is at its highest level in the program's thirty year history. The aging of the population combined with the increasing per capita cost of medical care for Medicare beneficiaries has seriously strained Medicare's resources. Total Medicare spending is projected to rise from $194.3 billion in 1996 to $468.7 billion by 2007, which represents an average annual rate of increase of 8.3 percent (United States House of Representatives, 1997). Policymakers are thus keenly interested in discovering ways to reduce health care expenditures for elderly Americans.

As part of this cost containment effort, the Health Care Financing Administration (HCFA) established what is commonly referred to as the Medicare risk program in 1985. Traditionally, Medicare has reimbursed health care providers on the basis of charges or costs. The risk program, which was legislated under Section 114 of the Tax Equity and Fiscal Responsibility Act of 1982 (TEFRA), introduces a system of prospective capitation for participating organizations. TEFRA authorizes Medicare to pay a capitated payment to qualified health maintenance organizations (HMOs) and other competitive medical plans (CMPs) which have a risk-based contract with HCFA. In exchange, the health plans assume responsibility for providing comprehensive health services to Medicare beneficiaries who choose to join them.

The creation of the Medicare risk program reflected persistent concerns over the economic incentives in fee-for-service reimbursement and growing interest in the potential for HMOs to reduce health care costs. In a fee-for-service system, the payment to providers increases with the quantity and intensity of services rendered. In contrast, in capitated systems providers are paid the same amount per member per month (adjusted, in some cases, by selected factors) regardless of the amount of medical services rendered to each patient. Consequently, there is an incentive to reduce the quantity of inputs into each episode of illness. This reduction may be achieved by improving the efficiency of production or by reducing the number and intensity of medical services rendered per episode of illness. Capitation also contains a theoretical incentive to provide services that may prevent or forestall the progression of disease (Centers for Disease Control, 1995).

HMOs have demonstrated an ability to provide care to the non-elderly population at a lower cost than traditional fee-for-service care arrangements by reducing unnecessary services and providing health services in the least expensive but appropriate setting. HMOs have been found to achieve a 28 percent reduction in total expenditures per enrollee relative to fee-for-service plans, primarily by reducing hospital admissions and hospital days by up to 40 percent (Manning et al., 1984). The Health Care Financing Administration hoped that TEFRA HMOs would be able to achieve similar savings for the Medicare population, though at the time of TEFRA's passage few studies had examined utilization patterns of the elderly in prepaid group practices (Greenlick et al., 1983; Weil, 1976; Iglehart, 1985).

In the last ten years, numerous attempts have been made to evaluate the success of the Medicare risk program in reducing the costs of care for elderly Americans. These analyses have consistently encountered problems of confounding due to the possibility of biased selection of Medicare beneficiaries into TEFRA HMOs. TEFRA allows Medicare beneficiaries to choose whether or not to enroll in a Medicare HMO. It is possible that those beneficiaries who choose to join an HMO and those who remain in a traditional plan differ significantly with respect to their health status, health care needs, or propensity to consume care. If this is the case, then observed differences in health services utilization in HMOs and fee-for-service plans may be due not to more efficient provision of care in one of the plans, but rather to baseline differences in their insured populations. If Medicare HMOs attract a disproportionate share of the relatively healthy Medicare population, then *favorable selection* can be said to have occurred. If Medicare HMO enrollees are, on average, less healthy than nonenrollees, then *adverse selection* can be said to have occurred in Medicare HMOs. Several studies of the early years of Medicare risk contracting found evidence of significant favorable selection into Medicare HMOs (Eggers, 1980; Eggers and Prihoda, 1982: United States General Accounting Office, 1986; Riley, Rabey and Kasper, 1989; Garfinkel et al., 1986; Brown, 1988). There is some evidence that favorable selection persisted through the late 1980s. (Lichtenstein et al., 1991, Hill and Brown, 1990; Riley, Lubitz and Rabey, 1991; Hill and Brown, 1992; Riley et al., 1996).

The possibility of biased selection is a significant policy problem not only because it complicates research efforts, but also because it may result in inappropriate capitation payments to HCFA's risk contractors. HCFA pays participating HMOs 95

percent of the estimated average amount that it would spend in fee-for-service reimbursements for a Medicare beneficiary residing in the same county. The adjusted average per capita cost (AAPCC) for a fee-for-service beneficiary is derived from a formula which factors in the costs of care in that county compared to the national average, as well as the beneficiary's age, gender, Medicare status (aged/disabled), institutional status, working aged status, and Medicaid eligibility. If biased selection occurs in Medicare HMOs, then the AAPCC-based payment will not be an accurate reflection of what the fee-for-service costs would have been for the HMO members. Specifically, if favorable selection exists in TEFRA HMOs, the HMO beneficiaries would have incurred FFS costs *lower* than the average per capita fee-for-service reimbursement and the AAPCC will *overcompensate* the HMOs. Consequently, HCFA will save less than the intended 5 percent of fee-for-service costs, and may actually lose money (Riley, Lubitz and Rabey, 1991).

1.2. Purpose of the study

In an effort to address this policy problem and to identify the true effect of HMO cost-containment mechanisms on medical care costs for the elderly, this study examines the extent to which biased selection exists in TEFRA HMOs in the 1990s and evaluates reductions in utilization achieved by TEFRA HMOs, controlling for selection bias. Three major research questions are investigated:

1. Is favorable selection still present in Medicare HMOs in the 1990s?

2. Does the amount of favorable selection in Medicare HMOs vary with the extent of market penetration achieved by Medicare HMOs in a county?

3. Do Medicare risk HMOs reduce the amount of health services used by Medicare beneficiaries relative to fee-for-service plans, controlling for selection bias?

With regard to the first and second questions, it is hypothesized that the amount of favorable selection will vary between counties in inverse relationship to their degree of market penetration, and that the overall level of favorable selection in Medicare HMOs will be significantly lower than the overall level in the early years of the Medicare risk program, when the HMOs collectively had a much smaller market share. The rationale for this hypothesis is fourfold. First, HMOs in markets with high managed care penetration are more likely than HMOs in low-penetration markets to seek to recruit Medicare beneficiaries whose health is relatively poor. Since there are only a limited number of low-risk individuals in the Medicare population, HMOs must eventually recruit higher-risk individuals in order to keep increasing their market share. Consequently, favorable selection can be expected to diminish as penetration increases.

Second, Medicare beneficiaries in relatively poor health will be more disposed to join an HMO in markets with high HMO penetration. Consumers in these mature markets are more likely than consumers in low-penetration markets to be familiar with the concept of managed care. This greater familiarity with how managed care works, along with the fact that these consumers are more likely to have family and acquaintances who are HMO members, may make them more disposed to join an HMO themselves. Additionally, Medicare beneficiaries in high-penetration markets are more likely to be aware that they have the option to join an HMO, and to be aware of the different companies offering service in their area, because of the greater amount of marketing and advertising by established managed care companies. Persons in high-

penetration markets are also more likely to have several HMOs to choose among, including open-panel HMOs which allow members to retain established relationships with physicians.

Third, rates of physician contracting with HMOs are considerably higher in areas with high managed care penetration than in low-penetration areas. Consequently, patients residing in high-penetration areas who have an existing relationship with a particular physician are less likely than patients in low-penetration areas to have to sever that tie in order to join an HMO. Since chronically ill individuals are especially likely to have strong physician ties, this effect operates to reduce the disincentives for Medicare beneficiaries in relatively poor health to join an HMO.

Fourth, markets with high Medicare HMO penetration tend to be markets in which Medicare HMOs have been operating for a long time. The enrollee population of HMOs in these markets will have aged considerably since their time of enrollment. Since elderly individuals are more likely to have health problems as they age, it is likely that any initial favorable selection in enrollment will dissipate over time, unless there is selective disenrollment.

With regard to the second research question, it is hypothesized that Medicare risk HMO enrollees will have slightly lower health services utilization than nonenrollees when sociodemographic and health status differences are controlled for. Studies of the early years of the risk program found that the HMOs achieved moderate reductions in inpatient and outpatient expenditures. However, since these studies did not completely control for selection bias, it is likely that some of this observed effect was actually due to favorable selection. That is, the coefficients on the HMO variables

in these studies likely were biased upward, capturing some of the effects that are actually due to health status variables. This study uses stronger statistical techniques to control for selection bias, and it is expected that the results will more accurately reflect the true impact of HMOs on utilization, i.e., the effects will be of smaller magnitude than those of earlier studies.

These hypotheses are investigated using 1993-1996 data from the Medicare Current Beneficiary Survey, the Bureau of Health Professions Area Resource File, and the Health Care Financing Administration's Medicare Market Penetration File and Prepaid Health Plans Monthly Report File. A system of simultaneous equations is estimated using panel data techniques and instrumental variables estimation. The results of the instrumental variables analysis are supplemented with a second analysis of the same equations using a discrete factor model, which is a semiparametric alternative for examining endogenous relationships.

The first equation in the simultaneous system models a beneficiary's choice to participate in a Medicare HMO or remain in a fee-for-service setting as a function of Medicare HMO market penetration and the individual's health status and sociodemographic characteristics. Statistically significant coefficients on the health status variables would suggest that biased enrollment into Medicare HMOs is occurring. Statistically significant coefficients on interactions of HMO market penetration with the health status variables would suggest that managed care penetration affects the extent to which selection bias occurs in a county.

The second, third, and fourth equations in the system test the effect of HMO membership on annual utilization. Specifically, effects on the probability of

hospitalization, the probability of having a physician visit, and the number of inpatient days given some inpatient use are examined. Utilization is modeled as a function of health plan choice, beneficiary characteristics, and geographic area characteristics. The simultaneous equations methodology takes account of the endogeneity which may exist between HMO choice and health services utilization and enables unbiased estimation of the effect of HMO cost-control mechanisms on utilization. A negative and statistically significant coefficient on the HMO variable in these equations would suggest that the HMO practice setting brings about reductions in health care utilization by Medicare beneficiaries even after controlling for differences in health status between HMO enrollees and non-enrollees.

1.3. Format of the dissertation

This dissertation is divided into nine sections. Section 2 reviews the existing literature on selection bias in TEFRA HMOs and provides a justification for the study. Section 3 sets forth the conceptual model used to examine selection bias and utilization and the economic theory underlying the model. Hypotheses and the empirical models that are used to test them are laid out in Section 4. The rationale for the analytical techniques proposed is also given. Section 5 consists of a description of the data sources and the methods used in creating the dataset. Estimation results are reported in Sections 6 and 7. Section 8 discusses the theoretical and policy implications of these findings. Finally, Section 9 summarizes the contributions and limitations of the study.

2. Background and significance

Several aspects of the health services literature are relevant to this study. This chapter provides background information about health maintenance organizations and their effects on health care utilization, the Medicare risk program and the effectiveness of HMOs for the Medicare population, and empirical evidence concerning biased selection into Medicare HMOs.

2.1. Health maintenance organizations

A health maintenance organization is an organization which assumes responsibility for the financing and delivery of a comprehensive package of health care services to a defined enrolled population in exchange for a prepaid capitated fee. HMOs thus serve as both insurers and health care providers. HMOs arrange for the provision of health services to their subscribers by entering into contracts with physicians, hospitals, and other providers, and subscribers generally face strong financial incentives to seek care only within the confines of these affiliations.

The specific relationships HMOs form with individual providers, and the reimbursement they give to them, may take a variety of forms. In a *staff model* HMO, the HMO employs physicians directly on a salaried basis. The physicians may also receive bonus payments based on their performance or productivity. Staff model HMOs are also called "closed-panel" HMOs because the HMO generally will not pay for care rendered to their subscribers by outside physicians (Wagner 1993). In *group*

model HMOs, the HMO contracts with a physician group practice to provide services to HMO subscribers but does not employ the physicians directly. Reimbursement to the physician practice may be on a capitated or cost basis. Group model HMOs are also closed-panel. *Network model* HMOs contract with a number of physician practice groups, typically on a capitated basis. Network models may be either closed-panel or open-panel (Wagner 1993). In *individual practice association model* HMOs, or IPAs, physicians in individual practices affiliate with one another as an IPA, and then contract with an HMO to provide services to subscribers for a capitated payment to the IPA. The IPA pays its member physicians on either a fee-for-service basis or a combination system in which specialists are paid on a fee-for-service basis and primary care providers accept a capitated payment. IPA physicians also see non-HMO patients, and the HMOs are open-panel. Finally, the *direct contract model* HMO forms contracts directly with individual physicians (Wagner 1993). Primary care capitation is commonly used, though physicians may be reimbursed on a fee-for-service basis. Direct contract model HMOs are open-panel.

HMOs have their historic origins in prepaid plans developed in the 1930s and 1940s by large employers and physician and consumer activist groups (Morrison and Luft, 1990). HMOs were limited in number and found almost exclusively in the western states until Congress passed the Health Maintenance Organization Act of 1973 (PL-222), which pledged financial support to HMOs which offered a minimum benefits package, charged premiums based on community-wide health care costs, and met certain other criteria (Alkire and Stolz, 1993). Amendments to the Act also provided that employers of twenty-five or more workers could be forced to offer an HMO option

to employees along with their regular fee-for-service offerings. This law, and the nearly $200 million in other federal money which was poured into HMO development over the next dozen years, provided a major impetus for HMO expansion. The expansion was further fueled by private industry initiatives to promote HMO market entry and substantial private investment in prepaid plans (Morrison and Luft, 1990). Total enrollment in HMOs has grown from 6 million in 1976 to over 59 million in 1995 (American Association of Health Plans, 1996).

Government and private interest in HMOs derives largely from a conviction that this form of health care financing and delivery can significantly reduce health care costs compared to traditional fee-for-service arrangements. HMOs have evolved a number of mechanisms to control both health care unit costs and utilization levels. Unit cost savings are achieved by selectively contracting with individual and institutional providers who agree to render services to HMO subscribers at a discounted rate. Large HMOs have significant bargaining power with providers and are able to negotiate volume discounts and deeper discounts than are offered to other insurers. HMOs employ a number of mechanisms to reduce overall utilization from the levels typically observed in fee-for-service plans. "Gatekeeping" by primary care physicians is commonly used to limit the number of specialist referrals and ancillary services. The use of salaried or capitated payment mechanisms with withholds and/or bonuses also provides physicians with incentives to limit the use of specialists and unnecessary tests, procedures, and other services. Precertification, mandatory second surgical opinion, case management, discharge planning, per diem hospital payments, and substitution of ambulatory for inpatient care may be used to limit the number of

hospital admissions and length of stay. In both inpatient and outpatient settings, concurrent and/or retrospective utilization review is undertaken by the HMO and physician practice profiles are used to identify and educate outlier physicians. Physicians may be excluded from participation in the HMO if their use of services is consistently too high. Overall, HMOs aim to achieve "efficiency in the mix of services and in the provision of any given service by an appropriate lowest cost provider" (Langwell and Hadley, 1989).

Empirical evidence suggests that the use of these cost-containment mechanisms has enabled HMOs to significantly reduce health care expenditures relative to fee-for-service care. In interpreting studies of utilization in HMOs it is important to be cognizant of possible problems of confounding due to favorable selection, discussed in greater detail below. In the absence of randomization, it is difficult to control for differences in the baseline health of HMO enrollees and fee-for-service subscribers. However, strong evidence of the cost-saving effects of HMOs comes from the RAND Health Insurance Experiment (Manning et al., 1984), a randomized controlled trial. A group of 1580 individuals was randomly assigned to receive care from either an HMO or a fee-for-service physician of their choice. Manning et al. determined that total expenditures were 28 percent lower in the HMO group than in the fee-for-service group. This difference was chiefly attributable to a 40 percent reduction ($p<0.01$) in hospital admissions and inpatient days in the HMO group.

Non-randomized studies provide only moderate support for the proposition that HMOs reduce hospital length of stay. Stern et al. (1989) found that controlling for age, sex, ER admission, diagnosis related group, and illness severity, overall length of

stay was 14 percent shorter for HMO patients than for fee-for-service, a difference which was significant at the 0.01 level, but that total costs were only 4 percent less. Other studies have been mixed (Luft, 1981; Wilner et al., 1981; Dowd, Johnson and Madson, 1986; Yelin et al., 1985). Luft's (1981) review of 26 studies of utilization in HMOs and fee-for-service arrangements concluded that HMO enrollees used 10 to 40 percent fewer hospital days than fee-for-service insureds, but that the difference was due to lower admission rates and not shorter length of stay. Many of the studies in Luft's review did not provide strong controls for health status, however. Adjusting for case-mix differences between HMO and fee-for-service plans is particularly problematic in studies of length of stay. Although favorable selection may exist in HMOs, it may also be that because HMOs encourage their physicians to treat patients in ambulatory settings if possible, HMO patients who are hospitalized may be more severely ill at the time of admission than are fee-for-service patients (Stern et al., 1989).

There is no clear evidence that HMOs effect significant reductions in the use of outpatient and physician services. The HMO and fee-for-service groups in the RAND study had a similar number of outpatient visits, though the HMO group had a greater number of visits specifically devoted to preventive care. The studies reviewed by Luft (1981) also failed to find a significant difference in the number of physician visits between HMO and fee-for-service patients.

The reasons for the lower rate of hospitalization among HMO enrollees are unclear. It is unlikely that the difference is attributable to selection bias because of the randomized design of the RAND study, though it is possible that persons in poor

health may have refused to participate in the study because they were unwilling to submit to randomization, so that the estimated effect does not apply to the full population. The smaller number of admissions may be due to the successful substitution of outpatient for inpatient care in the HMO, the successful use of preventive care by the HMO to reduce the incidence of illnesses requiring hospitalization, unnecessary hospitalizations by fee-for-service physicians, or inappropriate restrictions on admissions by HMO physicians. Other, non-randomized studies have found that lower rates of admission do not occur exclusively in discretionary hospitalizations (e.g., hospitalizations for laboratory tests or elective surgery), but are evenly distributed throughout all categories (Gillick, 1987; Wolinsky, 1981; Luft, 1981).

Some researchers have questioned whether the lower use of hospital care by HMOs results in poorer health outcomes for HMO patients. Studies have generally concluded that it does not. A randomized controlled trial involving 1673 individuals (Ware et al., 1986) found that there were no significant difference in any of the health outcomes studied, except for a subgroup of patients with poor initial health and low incomes. For this group, the fee-for-service patients had significantly fewer bed-days than the HMO patients ($p < 0.05$). Overall, the authors concluded that "there were no important deleterious or beneficial effects" of HMO membership. Brown and colleagues' (1993) study of Medicare HMOs found that rates of death, hospital, readmission, and post-admission complications among HMO and fee-for-service patients hospitalized for colon cancer or stroke were similar, and HMOs achieved these outcomes with significantly lower use of various procedures, tests, and services. There

also were no significant differences in outcomes for patients with chronic joint pain, urinary incontinence, or chest pain who received ambulatory service, despite the lower use of resources by the HMOs. Retchin and colleagues (1997) also found that, controlling for several patient characteristics, stroke patients in Medicare HMOs and fee-for-service settings showed no significant differences in survival patterns over a three-year period.

An earlier study of Medicare HMOs by the same authors (Retchin et al., 1992) also concluded that, after correcting for baseline differences between Medicare HMO enrollees and nonenrollees, the two groups experienced very similar patterns of decline in functional status. The study found no evidence that HMO enrollees faced more barriers to care than fee-for-service beneficiaries. Many other studies, including some randomized controlled trials, have produced similar findings (Clement et al., 1994; Lurie et al., 1992; Retchin and Brown, 1991; Langwell and Hadley, 1989). However, it should be noted that the recent Medical Outcomes Study (Ware et al., 1996) found that chronically ill Medicare beneficiaries receiving care in HMOs were more likely to experience a physical health decline over a four-year study period than similar beneficiaries in the fee-for-service sector. The Medical Outcomes Study is particularly notable for its long timeframe. The authors suggest that previous studies failed to detect a difference in health outcomes because the follow-up period was too short. Overall, though, there is a great deal of empirical evidence suggesting that health outcomes and the quality of care do not differ significantly in HMOs and fee-for-service settings, and that a Medicare beneficiary's health status trajectory is unlikely to be altered markedly as a result of joining an HMO.

2.2. Managed care and Medicare

2.2.1. The Medicare risk program

The Medicare risk program was adopted in response to escalating costs of care for Medicare beneficiaries in the fee-for-service setting. Total Medicare program payments have increased at an annual rate of approximately 14 percent since 1970, rising from $6.6 billion in 1970 to $168.1 billion in 1994 (Health Care Financing Administration, 1996a). Expenditures are projected to rise to $450.9 billion by 2005 (Burner and Waldo, 1995).

These cost increases have been driven in part by the aging of the population, which has meant more Americans on the Medicare rolls. The number of aged Medicare beneficiaries has grown from 19.1 million in 1966 to 32.4 million in 1994 (Health Care Financing Administration, 1996a). The Medicare population itself has also aged; a greater proportion of enrollees are now members of the "oldest-old" cohort (age 85+), while the 65-69 age group has been declining in numbers. This is significant because health care costs increase greatly with age. Elderly persons generate four times as much health care expenditures as younger persons (Waldo et al., 1989), and the "oldest-old" cohort has the highest per capita expenditures of all.

The Medicare risk program was seen as a possible solution to the Medicare cost crunch because of its potential to alter the structure of economic incentives facing providers. Moreover, it was anticipated that as the market penetration of Medicare HMOs increased, fee-for-service physicians would feel increasing pressure to enter into contracts with the HMOs that would change their practice patterns and pricing

strategies (Rossiter et al. 1989). In addition to reducing Medicare costs, the risk program was also seen as a way to provide Medicare beneficiaries with a wider range of health delivery system choices, similar to the array of choices enjoyed by younger, employed Americans.

The Health Care Financing Administration piloted two risk contracting demonstration projects before institutionalizing the risk program in TEFRA. The first series of demonstrations, called the Medicare Capitation Demonstrations, was initiated in 1978. Eight HMOs in five market areas participated in the demonstration beginning August 1980. Three of the Capitation Demonstrations were group model HMOs, two were staff model, two were network model, and one was an IPA model. All but one of the HMOs are still in operation today. The seven remaining Capitation Demonstration HMOs had 70,571 Medicare enrollees in January 1985. HCFA experimented with reimbursement methods during the Capitation Demonstrations; reimbursements to plans varied from 85 to 95 percent of the AAPCC and were linked to a number of risk-sharing arrangements (Trieger, Galblum and Riley, 1981).

The second series of demonstration projects, called the Medicare Competition Demonstrations, was a larger project involving 26 HMOs. The Competition Demonstrations began in August 1982 and continued until they converted to permanent risk-based contracts under TEFRA in April 1985 (Rossiter, Friedlob and Langwell, 1985). Nine of the Competition Demonstration HMOs were staff model plans, 4 were group model, 10 were IPA model, and 3 were network model. The Competition Demonstrations had enrolled 316,000 Medicare beneficiaries by 1986, when the project

was evaluated. The plans in this demonstration were all reimbursed with a prospective monthly payment equal to 95 percent of the AAPCC.

Based on its experience with the demonstration projects, HCFA formulated rules for the ongoing operation of risk contracting in TEFRA 1982 and its 1985 federal regulations. In order to obtain a TEFRA risk contract, a health plan must be either a federally qualified HMO or designated by HCFA as a Competitive Medical Plan (CMP). A CMP is functionally very similar to an HMO but is given wider latitude by HCFA in setting premiums than are HMOs. All contracting plans must have a minimum membership of 5,000 (or 1,500 if it is a rural plan), at least 1,000 of which must be at the subsidiary location for which the contract is being sought. The plan's membership must at all times consist of no more than 50 percent Medicare and Medicaid beneficiaries. The plan must have a 30-day open enrollment period each year, and may have other open enrollment periods or continuous open enrollment. During these enrollment periods the plan must enroll *any* Medicare beneficiary eligible to enroll under the rules of the program who lives in the plan's service area. Medicare beneficiaries with end-stage renal disease and those receiving care in a Medicare-certified hospice are not eligible to join an HMO. Enrollees are permitted to disenroll at the end of each month, and may not be involuntarily disenrolled by the HMO. Contracting health plans must agree to adhere to certain marketing rules, including prohibitions on discriminatory marketing (Zarabozo and LeMasurier, 1993).

TEFRA risk contractors are reimbursed by HCFA through a prospective capitated payment for each covered life calculated to be 95 percent of what HCFA would expect to pay for the beneficiary in a fee-for-service setting. The payment level

is calculated through a two-step process. First, an actuarial estimate of the average cost of care for a fee-for-service Medicare beneficiary residing in the same county is made. This county rate, called the adjusted average per capita cost (AAPCC), is equal to the following:

$$AAPCC_i = \text{Projected average Medicare cost per capita for the U.S.} \times \frac{\text{Historic Medicare costs per capita for county}_i}{\text{Historic Medicare costs per capita for the U.S.}}$$

where i is a county indicator. Second, the AAPCC is adjusted for certain beneficiary characteristics which may cause the beneficiary's health care costs to depart from the average costs in the county. The adjusters are the individual's age, gender, disability-related Medicare status, institutional status, employment status, and Medicaid eligibility. The adjusters account for the possibility that there may be biased selection into TEFRA HMOs based on these sociodemographic characteristics, but they do not account for residual bias.

Under the terms of TEFRA, if the capitated payment exceeds the projected cost of care for the HMO's Medicare enrollees (as estimated through the Adjusted Community Rate), the HMO is obligated to use the difference to provide additional benefits to subscribers and/or to reduce monthly premiums. The Adjusted Community Rate (ACR) is the HMO's determination of the premium it would charge for providing exactly the same package of benefits to a community rated group account (i.e. to non-elderly persons enrolled through their employer), adjusted to allow for the higher utilization of Medicare beneficiaries (Zarabozo and LeMasurier, 1993). The ACR includes the normal profit of a for-profit HMO.

Risk contractors are required to provide Medicare subscribers with coverage for all Part A and Part B services normally covered by Medicare. TEFRA also requires Medicare HMOs to offer benefits for home health care, mental health services, and substance abuse treatment, though CMPs are exempt from this mandate. Typically, TEFRA HMOs offer a package of services that is comparable to what a Medicare beneficiary with a traditional supplemental insurance or "Medigap" plan would receive. Since the HMOs are effectively competing with Medigap insurers for subscribers, they strive to offer a slightly more attractive benefits package than traditional Medigap at a lower out-of-pocket cost to beneficiaries. Like Medigap plans, TEFRA HMOs cover the Medicare deductibles and coinsurance for which a beneficiary is normally responsible. They may also provide coverage for any or all of the following "extra" services: prescription drugs, preventive care, immunizations, eye exams and eyeglasses, hearing exams and hearing aids, and dental care. Medicare HMOs typically offer first-dollar coverage for many of these services, permitting subscribers easy access to care without concern for costs (Brown and Langwell, 1988).

An additional benefit available to HMO enrollees is protection from "balance billing" by physicians who do not accept Medicare assignment. Traditional Medigap plans pay charges only up the Medicare allowable amount, leaving beneficiaries with liability for any charges above that cutoff. Although recent changes in the law now limit the maximum liability of a Medicare beneficiary for balance billing (Zarabozo and LeMasurier, 1993) and balance billing is becoming an increasingly rare practice among physicians, this benefit does provide an additional measure of security for HMO enrollees.

In order to receive this comprehensive package of services, HMO subscribers may have to pay a monthly premium out-of-pocket in addition to Medicare's monthly Part B premium. In 1996, one-third of TEFRA HMOs charged an additional premium, which averaged $13.52 per month. Eighteen percent of the plans charged a premium of $40.00 per month or more (Lamphere et al., 1997). The percentage of zero-premium plans has grown considerably, from 15 percent in 1986 to 63 percent in 1996. In those plans which do charge a premium, monthly premiums have fallen every year since 1991 (Health Care Financing Administration, 1996a). Moreover, TEFRA HMO premiums are generally lower than the premiums charged by traditional Medigap insurers.

All of the foregoing describes TEFRA risk contractors. It is important to note, however, that some HMOs contract with HCFA on a cost basis. Cost contracts provide that the HMO is reimbursed for the reasonable cost of all services actually provided to Medicare enrollees. In 1996, there were 85 cost contracts enrolling a total of 2 percent of Medicare beneficiaries, or 654,000 persons (Health Care Financing Administration, 1996a; Zarabozo, Taylor, and Hicks, 1996). This study focuses on risk HMOs rather than cost contractors due to the small enrollment in the latter and the fact that cost HMOs do not face the same structure of economic incentives as HMOs with risk-based contracts.

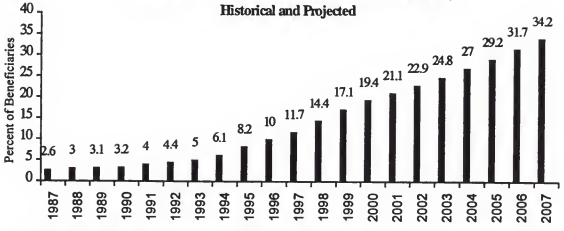
After a slow start, interest in the TEFRA risk program by HMO contractors and Medicare beneficiaries has begun to pick up markedly. The number of risk contractors grew from 87 in 1985 to 161 in 1987, then fell to 93 over the 1988-1991 period as some HMOs chose to exit the market. The 1992-1993 period saw a modest increase in

contracting, to 110 contracts. Since 1993 the number has more than doubled, to 248 contracts in January 1997. About two thirds of these risk contractors are IPA model HMOs.

The number of Medicare beneficiaries choosing to participate in the Medicare risk program has grown also. TEFRA HMOs cared for just 3.5 percent of the Medicare population (441,000 people) in 1985. The late 1980s saw slow rates of growth: 5.8 percent of Medicare beneficiaries were enrolled in HMOs at the end of 1990. Enrollment has increased dramatically since 1990, however; by June 1996, 11.2% of the Medicare population (3.6 million people) were enrolled in risk plans (Health Care Financing Administration, 1996a). From December 1994 through April 1996, Medicare HMO enrollment grew 53 percent. Between December 1993 and December 1994, enrollment grew 25 percent. In contrast, enrollment in private sector HMOs grew 13.1 percent over the same period (Zarabozo, Taylor, and Hicks, 1996).

Beneficiary interest in Medicare risk HMOs to is predicted to continue to grow steadily. The Congressional Budget Office projects that enrollment will reach 19.4 percent of the Medicare population by the year 2000 (United States House of Representatives, 1997); HCFA projects it at 22.9 percent for 2002 (Lamphere et al., 1997). Enrollment trends and projections for the next decade are summarized in Figure 2.1.

Figure 2.1.
Percent of Medicare Beneficiaries Enrolled in Risk HMOs, Historical and Projected



Source: Health Care Financing Administration, Office of Managed Care, "Medicare Managed Care Program Update, 1995" and "Medicare Managed Care Monthly Report", December 1991-December 1996; and Congressional Budget Office Medicare Baseline, January 1997. Reprinted in Lamphere et al. (1997).
Note: Figures to 1997 are actual data; 1997 and beyond are projections.

In addition to absolute growth in enrollment, the Medicare risk program has seen changes in the geographic distribution of enrollees over the last dozen years. While the demonstration project HMOs served only a few metropolitan areas, in 1996 TEFRA HMOs can be found throughout the United States. Forty-one states now have TEFRA risk contractors serving their Medicare populations, and 74 percent of Medicare beneficiaries live in areas served by TEFRA HMOs (Zarabozo, Taylor and Hicks, 1996). A choice between several different risk plans is available in many metropolitan areas, though rural areas continue to be underserved by risk contractors. The nine states still without Medicare HMOs are primarily rural with a low population density, are medically underserved according to indicators of physician-to-population ratios and availability of primary care, and have a low penetration of managed care among their general population (Lamphere et al., 1997).

Although most Medicare beneficiaries now have access to risk HMOs, enrollment continues to be heavily concentrated in a few geographic areas. The majority of the enrollment in Medicare HMOs is in four states: California, Arizona, New York, and Florida. Enrollment is highest in the Pacific census region due to the high managed care market penetration among the general population and the longstanding operation of HMOs in that area. TEFRA HMOs have also achieved high enrollments in the Mountain census region due to the large number of retirees living in Arizona (Health Care Financing Administration, 1992). In 1995, one-third of the Medicare beneficiaries residing in California, Oregon, and Arizona were HMO subscribers (Health Care Financing Administration, 1996a). In contrast, TEFRA HMO market penetration in several Midwestern and Southeastern states was less than 5 percent.

2.2.2. Effectiveness of HMOs for the Medicare population

Although studies of working-age individuals indicate that HMOs may substantially reduce some types of health care utilization, there are several reasons why results may be different for Medicare beneficiaries. The elderly population differs from younger populations in that the elderly have much higher average health care costs and a somewhat less skewed distribution of costs (Gruenberg, Tompkins and Porell, 1989). Elderly individuals are more likely than younger persons to die, to have a high-cost illness and to be functionally impaired. Furthermore, HMO cost-containment mechanisms such as substitution of ambulatory for inpatient care may not be as easy to accomplish for elderly patients. Bates and Connors (1987) and Siu,

Brook, and Rubenstein (1986) note that home or outpatient care of elderly patients is often problematic because these patients may have multiple comorbidities and disabilities and may lack strong social support at home.

What does the empirical evidence show regarding the effectiveness of HMOs in reducing costs for elderly patients? A study of nine HMOs operating in the early years of the Medicare risk program found that the HMOs studied achieved a modest reduction in hospital use (Langwell and Hadley, 1989). Although no reductions were observed in the first year of study, reductions of 14 to 25 percent were observed in the second year, for an overall cost savings of about 8 percent. The savings was due to fewer admissions rather than shorter length of stay. Another study of 22 Medicare HMOs (Nelson, Rossiter, and Adamache, 1986) also found that hospital use rates were considerably lower in the HMOs than among fee-for-service beneficiaries, but the study did not provide strong control for differences in the baseline health of the two groups. A more recent study (Brown et al., 1993) reported that Medicare HMOs shortened the average hospital length of stay by 16.8 percent relative to fee-for-service, but did not reduce the number of admissions.

The effects of Medicare HMOs on outpatient utilization appear to be complex. Brown and colleagues (1993) found that, controlling for possible differences in health status between HMO enrollees and non-enrollees, HMO enrollees had a 5 percent higher probability of seeing a physician at least once during the year, but a slightly lower probability of having one or more physician visits per month. The authors estimated that overall, Medicare HMOs spent about 10.5 percent less for all Medicare-covered services combined than the Medicare would have spent if the HMO enrollees

had remained in fee-for-service settings. It must be remembered, however, that some of this cost savings could be due not to reductions in utilization rates, but to the HMOs having obtained provider discounts. The available evidence suggests that the reductions in utilization which were achieved did not result in lower quality care in the HMOs. There were no significant differences in health outcomes such as mortality rates, rates of hospital readmission, rates of post-admission complications, and access to care between HMO and fee-for-service patients, once differences in health status were controlled for (Brown et al., 1993; Langwell and Hadley, 1990). In summary, evaluations of the Medicare risk program suggest that HMOs are moderately effective in reducing utilization and costs for elderly individuals without sacrifice of quality of care.

Discussions of utilization reductions Medicare risk HMOs raise a question as to whether the reductions might actually be attributable to differences in the baseline health status of HMO enrollees and non-enrollees, rather than to HMO cost-containment mechanisms. A large number of studies have examined the sociodemographic and health status profile of the Medicare HMO and fee-for-service populations, and have found evidence that HMO joiners are in better health than those individuals who choose to remain in fee-for-service settings. These research findings are discussed below.

2.3. Biased selection in managed care plans

2.3.1. The concept of biased selection

Biased selection into HMOs is said to occur when two conditions are present: (1) HMO enrollees are not representative of the larger population on some characteristics which are thought to be associated with health services utilization; and (2) those differences are not taken into account in the formula by which the HMO is paid. Condition (1) reflects an assumption that individuals are generally able to make a choice as to whether or not to join an HMO, and that the choice they make reflects, among other things, their perceived risk of needing various kinds of health services. This perception of risk is formed based on information they have about various personal characteristics which might dispose them to use (or not use) health services. Information about past health care use, overall health status, specific health problems, functional status, degree of health worry, attitudes towards organized medicine and physicians, propensity to access the medical care system, and other characteristics all inform individuals' health insurance choices. In most cases, however, detailed information about these characteristics is not available to insurers (Farley and Monheit, 1985). Consequently, HMOs are not able to evaluate perfectly the risk represented by each enrollee and accordingly receive adjusted capitation payment levels. Condition (2) reflects this problem.

In the case of TEFRA HMOs, the AAPCC-based payment formula incorporates risk adjusters only for age, gender, disability status, institutional status, employment status, and Medicaid eligibility of the enrollee. HMOs receive a slightly higher capitation payment for caring for individuals whose profile on these characteristics

suggests that the expected value of their health care expenditures will be higher than average, and a slightly lower payment for beneficiaries with more favorable profiles. A finding that TEFRA HMO enrollees differ systematically from fee-for-service Medicare beneficiaries on these characteristics would *not* constitute evidence of selection bias, because condition (2) is not satisfied. Disproportionate enrollment of beneficiaries on characteristics included in the payment formula is not selection bias, but "selectivity" (Hornbrook, Bennett, and Greenlick, 1989), and is not a policy problem.

In contrast, a significant policy problem does occur when an *omitted* risk factor is correlated with the choice of health plan (HMO or fee-for-service), so that a difference in predicted health care expenses between the two groups arises which is not accounted for by the payment formula (Wilensky and Rossiter, 1986). When the risk profile of HMO enrollees is better than that of fee-for-service beneficiaries on some variable(s) omitted from the payment formula, favorable selection into HMOs is said to occur. When the risk profile of HMO enrollees is worse than that of their fee-for-service counterparts, adverse selection occurs. As is discussed below, favorable selection into HMOs appears to be much more common than adverse selection.

In theory, favorable selection into HMOs could occur in two ways: the HMO could refuse to accept individuals with poor risk profiles, or individuals could self-select into managed care plans. Since TEFRA prohibits Medicare HMOs from denying membership to any eligible Medicare beneficiary, the first explanation is unlikely to provide much insight into selection bias in TEFRA HMOs. Certain of the risk contractors in the demonstration projects were not subject to this prohibition, however.

Some commentators have noted that TEFRA HMOs may still be able to passively promote favorable selection. Brown (1988) showed that group and staff model HMOs tended to enroll a healthier population of Medicare beneficiaries than IPA models because group and staff model plans usually require that enrollees sever existing physician relationships, and individuals with chronic illnesses are more likely to be unwilling to do this. Thus, HMOs may court a healthier member population simply through their choice of organizational form (Manton and Stallard, 1992). They may also encourage favorable selection through the benefits package they choose to offer and through careful selective marketing. Self-selection by enrollees, however, is probably most responsible for any selection bias which exists in TEFRA HMOs. A thorough discussion of the considerations which might lead relatively healthier Medicare beneficiaries to join HMOs is provided in Section 3.1.

Before examining the empirical evidence concerning the degree of biased selection which exists in HMOs, it is helpful to have an understanding of the methodologies commonly employed in selection bias studies. These studies typically have employed four types of indicators of biased selection: differences in pre-enrollment health services utilization, differences in post-enrollment mortality, differences in sociodemographic variables associated with health services use, and differences in self-reported health status. Each of these measures has shortcomings. Studies of pre-enrollment utilization have been criticized because much of individuals' health services use is due to acute, transitory health events. Because of this transitory component of health, utilization varies widely from year to year, and prior use is not highly predictive of future use (Lichtenstein et al., 1991; Welch, 1985). Post-

enrollment mortality rates are a crude indicator of selection bias due to the possibility that differences in mortality rates may be due to differences in the quality of care in HMOs and fee-for-service settings rather than differences in the baseline health of their enrollees. Studies comparing HMO and fee-for-service enrollees on sociodemographic characteristics believed to be associated with health care use suffer from the problem that many of the sociodemographic characteristics are not consistently found to be predictive of utilization. Finally, studies using self-reported health status as an indicator of selection bias are limited by the measurement error inherent in most health self-reporting instruments. Perceived health status may not accurately reflect an individual's true health status, respondents may erroneously report the presence or absence of important health conditions, and so on. However, health status self-reports are a fairly good indicator, compared to the alternatives, because many aspects of health status are objectively measurable and can be validated, and because perceived health status and degree of health worry may be just as important in explaining propensity to consume health care services as a person's true health status.

In the review of the literature concerning biased selection in HMOs which follows, studies of the working-age population are treated separately from studies of Medicare HMOs. Research findings relating to Medicare HMOs operating during the two demonstration projects are also distinguished from more recent findings. These distinctions are made in order to account for important differences in the characteristics of the health plans and enrollees being studied. In interpreting the evidence regarding differences in the composition of HMO and fee-for-service plan memberships, it is important to remember that these differences only constitute selection bias if (1) they

are also related to utilization, and (2) they are not accounted for in the payment formula. Empirical findings concerning the influence of the factors discussed below on health services utilization are presented in Section 3.2.

2.3.2. Previous studies of biased enrollment of the non-elderly in HMOs

A large number of studies have analyzed the characteristics of working age persons who elect to join HMOs. The overall conclusion which can be drawn from this body of research is that findings concerning the existence of favorable selection have been mixed. Studies of the working age population that have examined pre-enrollment utilization as an indicator of selection bias have generally concluded that favorable selection exists on this measure. Jackson-Beeck and Kleinman (1983) studied 11 companies in the Twin Cities area and found that inpatient utilization prior to enrollment was 53 percent lower among HMO enrollees than among those who retained fee-for-service coverage, a difference which was statistically significant at the 1 percent significance level after controlling for age. Schuttinga, Falik and Steinwald (1985), who examined 8,500 federal employees, determined that those who chose HMO plans had lower prior use of hospitalization, surgery, mental health services, and dental services, though their use of maternity services and chronic visits was no lower than fee-for-service subscribers. Strumwasser and colleagues' (1989) analysis of 48,379 employees in Michigan determined that workers who switched to an HMO plan had 54 percent lower health care expenditures in the year prior to the switch than those who remained in fee-for-services ($p < 0.001$). Favorable selection was apparent in the rate of hospital admissions, the average length of stay, and the number of outpatient

visits and services used by HMO enrollees prior to switching. Merrill, Jackson and Reuter (1985) and Buchanan and Cretin (1986) also found evidence that HMO joiners had lower prior utilization. However, earlier studies by Berki et al. (1977) and Tessler and Mechanic (1975) found no differences in rates of pre-enrollment hospitalization between HMO joiners and non-joiners.

Findings have varied as to the relevance of sociodemographic characteristics for predicting HMO choice. In an early study, Tessler and Mechanic (1975) found that city employees who chose an HMO plan were better educated and more likely to be unmarried than those who remained in fee-for-service. Juba, Lave and Shaddy (1980) also determined that education was positively related to HMO choice. Dowd and Feldman's (1985) study of 5,161 employees in the Twin Cities area found that HMO joiners were younger than those who remained in fee-for-service plans. Studies by Jackson-Beeck and Kleinman (1983) and Taylor, Beauregard and Vistnes (1995) yielded similar findings with regard to age.

Taylor, Beauregard and Vistnes' (1995) analysis of data from the 1987 National Medicare Expenditure Survey also determined that HMO joiners were significantly more likely to be nonwhite and to reside in a metropolitan area, though this may be due as much to HMO location decisions as to individual enrollment decisions. The same authors found that observed differences on marital status and education were not statistically significant. Mechanic, Ettel and Davis (1990) studied 296 university employees and, despite the small sample size, found that employees who chose an HMO plan were more likely to be older and Caucasian. These results conflict with Taylor, Beauregard, and Vistnes' findings. In their 1980 literature review, Berki and

Ashcraft noted that the effect of race on insurance choice is unclear; some studies have found that nonwhites are more likely to join HMOs (Roemer et al., 1972), while others have found no racial differences between enrollees and nonenrollees (Moustafa, Hopkins, and Klein, 1971; Tessler and Mechanic, 1975).

With regard to the effect of income, Grazier and colleagues (1986) found that among Washington state employees, lower income workers were more likely to join an HMO than those with higher incomes. Buchanan and Cretin (1986) and Juba, Lave and Shaddy (1980) found the same effect. Welch and Frank (1986) also found that income was negatively related to HMO choice, but their study is problematic in that it did not exclude individuals who did not have the option of joining an HMO.

Research findings are ambivalent with respect to the health status of HMO joiners relative to fee-for-service stayers. The diversity of results may well be attributable to the researchers' use of different measures of health status; for example, self-perceived health status, degree of health worry, number and type of chronic conditions, health risk factors, and functional status and disability measures. Tessler and Mechanic's early (1975) study found no evidence of favorable selection on perceived health status, bed disability, major illness, or hospitalization either when these variables were examined separately or when they were examined as part of an index of overall family risk. However, the sample size was relatively small (n=333) and the null results could have been due to a lack of statistical power. Other studies in the 1970s also failed to detect any biased selection on health status (Berki et al, 1977; Scitovsky, McCall and Benham, 1978).

More recently, a multivariate analysis by Taylor, Beauregard and Vistnes (1995) also concluded that there was little evidence of favorable selection on any of the health status indicators studied, including perceived health status, bed days, work limitations, and number of chronic conditions. In contrast, Dowd and Feldman (1985) identified a negative relationship between the presence of chronic health problems and HMO choice. Juba, Lave and Shaddy (1980) found complex effects concerning health status: the number of family members reporting a chronic illness had a positive and significant effect on the probability of choosing an HMO, but the average self-reported health status of family members bore a significant negative relationship to HMO choice. The authors speculated that perhaps the *type* of illness present in the family influenced HMO choice. Hellinger concluded in his 1987 literature review that the health status of HMO joiners is not significantly different from that of fee-for-service enrollees.

To summarize the foregoing, there does not exist clear evidence for the favorable selection hypothesis among the working age population in private HMOs. While most studies have found evidence that HMO joiners had lower pre-enrollment health services utilization than individuals who stayed in fee-for-service plans, little consensus has emerged about whether private HMO enrollees are in better health as measured by various types of health status self-reports or whether HMO enrollees differ systematically from non-enrollees on sociodemographic characteristics which predispose people to lower health care use. Recent studies find evidence of favorable selection more often than studies from the 1970s, but when considered as a whole, the body of research on this issue is not conclusive.

The literature regarding selection effects in Medicaid HMOs is also relevant. As of June 1996, 36 states had programs providing for the voluntary or mandatory enrollment of Medicaid beneficiaries in HMOs (48 states had programs involving HMOs or other types of managed care plans). Nationwide, about 40 percent of Medicaid beneficiaries, or about 13.3 million people, are enrolled in some type of Medicaid managed care plan (Health Care Financing Administration, 1996b).

Evaluations of Medicaid HMOs have frequently but not uniformly found evidence of favorable selection in HMO enrollment. DesHarnais' (1985) analysis of 35,933 Medicaid AFDC families in Wayne County, Michigan found that families with very low pre-enrollment utilization enrolled in HMOs in disproportionately large numbers. The author speculated that this phenomenon was due to the reluctance of those with regular contacts with physicians to sever their physician ties in order to join an HMO. Davies (1989) compared a sample of Medicaid beneficiaries in a capitated prepaid health plan to a sample in the fee-for-service sector and found that the prepaid health plan group was significantly younger, better-educated, and in better health than the fee-for-service group. Iglehart (1995) observed that the principal groups enrolled in Medicaid managed care plans have been children and adults in AFDC families, rather than the elderly, blind, or disabled. This indicates favorable selection, since AFDC mothers and their children are far less expensive to care for than elderly, blind, or disabled individuals. A recent study by Sisk and colleagues (1996) of Medicaid beneficiaries in five managed care plans in New York determined that managed care enrollees were better educated and reported better health status than their fee-for-

service counterparts. This finding was unsurprising given that New York State had a policy of not encouraging people with chronic conditions to enroll in HMOs.

In contrast to these results, two studies have found either neutral or adverse selection into Medicaid HMOs. Researchers evaluating the Medicaid Competition Demonstrations (Freund et al., 1989) found no evidence of selection bias (measured by pre-enrollment utilization) in either direction in the two HMOs for which this issue was studied. In a 1996 study of a Florida staff model HMO, Buchanan, Leibowitz and Keesey (1996) determined that the plan actually attracted somewhat sicker than average enrollees. The evidence for favorable selection into Medicaid HMOs, therefore, is strong but not uncontroverted.

Studies of biased selection in Medicaid HMOs, while useful for providing empirical support for theories of health plan choice, are not directly generalizable to Medicare HMOs. There are several important differences in the Medicare and Medicaid populations and risk program rules. First, some state Medicaid programs make participation in a Medicaid managed care organization mandatory for all beneficiaries residing in certain areas. While selection bias may still occur in situations of mandatory enrollment in that beneficiaries in good and poor health may distribute themselves unevenly among different health plans (Freund and Hurley, 1995), the selection dynamics are not the same as in situations where participation is voluntary. Second, the organizational forms and financial incentives of Medicaid HMOs vary significantly between states, with many departing markedly from the risk HMO models seen in the Medicare program. Third, while all Medicare beneficiaries except those with pre-existing end stage renal disease are allowed to enroll in HMOs and the HMOs

are prohibited from denying membership to anyone, many state Medicaid policies openly discourage enrollment of chronically ill beneficiaries in Medicaid HMOs. Finally, because Medicaid beneficiaries generally do not face premium differentials or out-of-pocket costs regardless of whether they choose an HMO or a fee-for-service plan, their health plan choice process may be quite different from that of Medicare beneficiaries (DesHarnais, 1985). Medicaid beneficiaries may also assume a more passive role in plan choice decisions due to low education, a lack of sophistication, low expectations, and the type of docile behavior encouraged by welfare agencies (Spitz, 1979). Thus, caution should be used in extending the results of studies of Medicaid HMOs to the Medicare risk program.

The evidence concerning favorable selection into HMOs by Medicare beneficiaries is a great deal more compelling than studies of the employed population. It is to this evidence that we now turn.

2.3.3. Previous studies of selection bias in Medicare HMOs

In reviewing the large number of studies which have examined the extent to which biased selection exists in Medicare HMOs, it is helpful to divide the studies into those evaluating demonstration project HMOs over the 1975-1985 period and those examining TEFRA HMOs. As is discussed in Section 2.4.2., there are a number of reasons why the enrollee composition of demonstration HMOs may be different from that of HMOs operating today.

A comprehensive evaluation of the Medicare Competition Demonstrations was performed by Mathematica Policy Research, Inc. and the Medical College of Virginia

(MCV), under contract from HCFA (Brown, 1987; Brown, 1988; Langwell and Hadley, 1989). This evaluation produced evidence of favorable selection into the demonstration HMOs on prior utilization, sociodemographic characteristics, and self-reported health status. Administrative data from the time of program start-up through December 1984 for a random sample of 1,000 in 17 HMOs and 1,000 nonenrollees in the same geographic areas revealed that HMO enrollees in 13 of the 17 HMOs had experienced significantly lower levels of utilization prior to enrollment (Brown, 1987). These differences persisted even after adjustment for the AAPCC risk factors. Prior utilization across the 17 plans averaged 21 percent less for HMO enrollees than for fee-for-service beneficiaries.

These results supported earlier findings by Eggers (1980) of favorable selection on prior use in two of the HMOs in the Medicare Capitation Demonstrations. After performing the AAPCC adjustments, Eggers found that pre-enrollment inpatient utilization was about 25 percent less for the HMO enrollees than for the fee-for-service beneficiaries. Outpatient and physician reimbursement was about 12 percent less. A continuation of this study (Eggers and Prihoda, 1982) also produced evidence of favorable selection on both prior inpatient and prior outpatient use in two of the Capitation Demonstrations, and favorable selection on just outpatient use in a third HMO. Kasper and colleagues (1988) also examined the Capitation Demonstration HMOs and found an even larger selection effect: pre-enrollment reimbursements were 42 percent lower among HMO enrollees at one of the demonstration projects than among their fee-for-service counterparts, and at two of the HMOs, enrollee admission rates were significantly lower than for non-enrollees.

In addition to this favorable selection on pre-enrollment utilization, it appears that demonstration HMOs also enjoyed a degree of favorable selection on post-enrollment mortality rates. At three of four Capitation Demonstration HMOs studied, enrollees had significantly lower mortality in the year following enrollment, controlling for age, gender, Medicaid eligibility, and institutional status (Riley, Rabey and Kasper, 1989). However, in the second year post-enrollment, enrollee mortality increased substantially and was not significantly different from that of fee-for-service beneficiaries at two of the HMOs. Over the entire 24-month period post-enrollment, the relative risk of death for enrollees at one site (Greater Marshfield Community Health Plan) was 0.68, and at another (Fallon Community Health Plan) was 0.40 (Kasper et al., 1988).

Other studies support this finding of lower mortality rates among demonstration HMO enrollees compared to nonenrollees. A study by the U.S. General Accounting Office (1986) of 27 HMOs participating in the Competition Demonstrations determined that mortality rates were significantly lower for the HMOs than for fee-for-service plans. The Mathematica/MCV evaluation of Competition Demonstration HMOs concluded that mortality was lower in all 17 demonstrations studied, though the difference was only significant at 12 of the sites (Langwell and Hadley, 1989). The enrollee mortality rate was approximately 25 percent lower than the rate for nonenrollees.

There are strong indications of favorable selection into demonstration HMOs on sociodemographic characteristics which are predictive of health services utilization. Again, selectivity on age, gender, institutional status, Medicaid eligibility, employment

status, and disability status does not constitute a policy problem because these risk factors are included in the AAPCC formula. Indeed, Medicare HMO enrollees do differ significantly from nonenrollees on most of these characteristics, particularly age (Retchin et al, 1992; McCombs, Kasper and Riley, 1985; Nelson, Rossiter and Adamache, 1986; Hornbrook, Bennett, and Greenlick, 1989; Langwell and Hadley, 1989). However, they also appear to differ on income and marital status (Retchin et al., 1992; Langwell and Hadley, 1989), which are omitted from the payment formula but may be correlated with health status and propensity to use health care services. The relationship of income to HMO participation is unclear; significant associations have been identified in both the negative (Brown and Langwell, 1988) and positive (Rossiter et al., 1989) directions. Retchin and colleagues (1992) failed to detect any differences on education, race, or gender.

There is persuasive evidence that demonstration HMO enrollees had better self-reported health status than their fee-for-service counterparts. The Mathematica/MCV evaluation found favorable selection on self-perceived health, functional disability measures, and bed days (Brown, 1988) in Competition Demonstration HMOs. HMO enrollees were also less likely to be aware of a health problem they thought might require hospitalization (Langwell and Hadley, 1989). They were, however, more worried about their health than were the fee-for-service beneficiaries. Retchin and colleagues (1992) confirmed that there were significant differences in terms of health problems that might require medical care, the number of bed days and restricted activity days, ability to perform the activities of daily living (ADL) and the instrumental activities of daily living (IADL), with HMO enrollees appearing healthier

than fee-for-service Medicare beneficiaries. Studies of the earlier Capitation Demonstrations also found that HMO enrollees had more positive perceptions of their health status and fewer functional limitations than non-enrollees (Kasper et al., 1988). Every study of demonstration HMOs reviewed in the preparation of this paper found evidence of favorable selection on some aspect of self-reported health status.

The studies described above all examined Medicare HMOs during the demonstration projects conducted from 1978 to 1985. Since the passage of TEFRA and the large expansion in both the number of risk contractors and the number of Medicare beneficiaries enrolled in HMOs, seven studies have examined selection bias in Medicare HMOs. Hill and Brown (1990) analyzed 1986 and 1987 Medicare administrative data from each of the 98 TEFRA risk HMOs operating at that time. The researchers found that pre-enrollment utilization by HMO members was 24 percent lower than that on fee-for-service beneficiaries, and that HMO members were 24 percent less likely than fee-for-service patients to have been hospitalized with a diagnosis that was predictive of high future costs, as determined by the diagnostic cost group (DCG) methodology developed by Ellis and Ash (1988). The authors concluded that about two-thirds of the plans analyzed experienced clearly favorable selection, while the remaining one third experienced slightly favorable or neutral selection (Brown et al., 1993). The study thus provides support for the hypothesis that favorable selection persisted in Medicare HMOs in the late 1980s, at least as measured by pre-enrollment utilization.

Riley, Lubitz, and Rabey (1991) examined the mortality rates of all Medicare beneficiaries in the 108 TEFRA HMOs operating in 1987. After adjusting for the

AAPCC risk factors, the authors determined that the adjusted mortality ratio for HMO enrollees compared to fee-for-service beneficiaries was 0.80. The ratio was significantly different from 1 ($p<0.001$). This finding was suggestive of strong favorable selection in the HMOs.

Lichtenstein and colleagues (1991) collected survey data on 6,303 Medicare beneficiaries in 1988, including randomly selected samples of enrollees in 23 TEFRA HMOs and fee-for-service beneficiaries in the same market areas. The researchers regressed functional status (an 8-category Guttman Scale) on age, sex, and HMO/fee-for-service membership, and found that 8 of the 23 plans studied had memberships with significantly better functional status than nonenrollees. For 15 plans, no significant difference was observed. The authors also conducted t-tests of the mean difference in functional status between enrollees and nonenrollees, adjusting for age and sex, and found significant mean differences in 9 plans. Another 10 plans were determined to have experienced favorable selection in one or both tails of the functional status distribution. Lichtenstein and colleagues' findings support the hypothesis of continuing favorable selection in TEFRA HMOs.

Dowd and colleagues (1994) compared TEFRA HMO enrollees in the Twin Cities to nonenrolled Medicare beneficiaries using 1988 survey data. The total sample size was 2,377, of which 877 were HMO members. A multiple regression model of HMO choice with a number of health status and sociodemographic variables as regressors produced several interesting findings. Income had a highly significant, positive effect on the probability of choosing an HMO. Education was not strongly associated with plan choice. HMO enrollees were significantly more likely to be

married. However, the differences between HMO and fee-for-service enrollees on the observed health status measures were not large. None of the coefficients on the 18 included health condition variables was statistically significant at the 0.05 level. The study provided little support for the hypothesis of favorable selection on health status variables.

Hill and Brown (1992) surveyed 12,857 Medicare beneficiaries, about half of whom were enrolled in 76 different TEFRA HMOs, in April 1990. Results indicated that HMO enrollees were healthier and had fewer functional impairments than nonenrollees, as measured by self-perceived health status, presence of 6 specific health conditions, and bed days. Even after controlling for the AAPCC risk factors, HMO members were 15 to 30 percent less likely than fee-for-service beneficiaries to report having the health conditions studied (Brown et al., 1993).

Price Waterhouse (1996) analyzed health status differences between Medicare fee-for-service and HMO beneficiaries using 1992 Medicare Current Beneficiary Survey (MCBS) data. Health status measures were used to predict the cost of care for HMO enrollees had they remained in the fee-for-service sector, controlling for the AAPCC adjustment factors. These imputed costs were then compared to the predicted costs for fee-for-service stayers. In contrast to many other studies, this study found no predicted cost differences between HMO enrollees and non-enrollees. This study, however, did not use 1992 data.

Finally, Riley and colleagues (1996) recently published a comparison of the health status of 863 Medicare HMO enrollees with that of 4,576 fee-for-service beneficiaries using 1994 MCBS data. In order to determine whether HMO enrollees

were healthier than non-enrollees, the authors fitted several logistic regression models with health status measures as the dependent variable and HMO membership, metropolitan statistical area, and the AAPCC adjustment factors on the righthand side. The researchers found that HMO members were significantly more likely than non-enrollees to report their health as fair or poor. There were also significant differences in the number of reported ADLs and IADLs. Differences in the likelihood of reporting a diagnosis of twelve chronic conditions, however, were not significant with the exception of heart disease. Riley and colleagues also examined the potential impact on Medicare payments of including health status adjusters to the AAPCC by estimating predicted "cost ratios", defined as the ratio of an individual's cost to average Medicare costs. Cost ratios were predicted using a model developed by Gruenberg, Kaganova, and Hornbrook (1996) which includes sociodemographic and health status variables. The results suggested that the average predicted cost for HMO enrollees was only 0.84 (compared to 0.99 for fee-for-service beneficiaries). The authors estimated that the inclusion of these adjusters in the AAPCC formula would reduce the AAPCC by 12 percent.

The Riley study is worthy of discussion because its purpose and data are very similar to those of the present study. It is a significant study in that it reports on 1994 MCBS data, which are more recent data than have been analyzed in any other study. It is important to note, however, that Riley and colleagues' study answers different questions than the present study. First, with regard to favorable selection, Riley examines the *correlation* between health status and HMO membership; the present study tests a *causal* model of HMO choice. That Riley's model is not a causal model is

evidenced by the fact that HMO membership is on the righthand side, and the dependent variable is health status. Moreover, the text of his report contains no suggestion that he is attempting to test a theory of HMO choice. His model is appropriate for the purpose for which it is employed, but Riley's results cannot be used to draw inferences about the causal influence of health status on HMO choice.

Second, Riley's purpose is not to measure the effect of HMOs on health services utilization, but rather to test the effect on Medicare payments of including more health status adjusters in the AAPCC formula. He employs the results of a study by Gruenberg, Kaganova, and Hornbrook (1996), which used 1991 MCBS data to regress actual utilization on a host of health status and sociodemographic variables. Riley uses the beta coefficients from this regression to compute predicted utilization costs for HMO and fee-for-service individuals using 1994 MCBS health status and sociodemographic data. Importantly, he does not employ *actual* utilization data from 1994 (he notes that predicted rather than actual costs were used because the necessary claims data were not yet available at the time of the study). He first computes the ratio of individual predicted costs to average predicted costs separately for HMO members and fee-for-service individuals, and takes the averages of these two sets of ratios. He then derives an enhanced AAPCC ratio by computing the ratio of the two averages. This methodology is entirely appropriate for the purpose for which Riley employs it. It does not, however, tell us anything about the effect of HMOs on utilization costs. To do this, one must regress *actual* utilization data on HMO membership and control variables.

In summary, recent studies of Medicare HMOs operating after the passage of TEFRA have generally found evidence of continued favorable selection, though the Price Waterhouse (1996) and Dowd (1994) studies did not. Most of these studies analyzed data from 1988 or earlier. Only three studies (Hill and Brown, 1992; Riley et al., 1996; Price Waterhouse, 1996) have examined data from 1990 or later. None of the studies used longitudinal data. Based on the empirical research conducted to date, HCFA believes that favorable selection persists and, in fact, is getting stronger (Vladeck, 1997).

2.4. Significance of the research

2.4.1. Policy significance of favorable selection

Ascertaining the extent to which favorable selection occurs in TEFRA HMOs today is extremely important for two reasons. First, the available evidence suggests that HCFA is currently overpaying its risk contractors due to favorable selection. The AAPCC-based capitation formula assumes that Medicare HMOs are caring for a population of beneficiaries which has the same risk profile as fee-for-service beneficiaries, except for differences on age, gender, institutional status, Medicaid eligibility, employment status, and disability status. However, research has demonstrated that the HMO enrolled population differs from the fee-for-service population on several factors omitted from the payment formula which are predictive of high health services utilization. The expected level of their health care utilization is actually considerably lower than that of their fee-for-service counterparts. Consequently, the AAPCC formula will overcompensate the HMOs for the provision

of their care, at least in the short run. It will save less than the 5 percent intended, and may actually cost more than HCFA would have paid had the HMO enrollees remained in fee-for-service settings. If the magnitude of selection effects and the specific characteristics on which HMO enrollees differ from fee-for-service beneficiaries could be determined using recent data, then HCFA's payment formula could be altered to account for this selection bias. Further risk adjustment is desirable not only in order to save money, but also to encourage Medicare HMOs to compete on the basis of quality and efficiency rather than on attracting the healthiest enrollees (Ellis et al., 1996).

Estimates of the extent of overpayment have varied widely. A study by Whitmore and colleagues (1989) used 1980 National Medical Care Utilization and Expenditure Survey data to perform a simulation of reimbursement bias in the AAPCC formula. The researchers divided their sample of 6,000 individuals into categories based on their values on four health status variables not included in the AAPCC formula (perceived health status, activity limitations, functional limitations, and diagnosis of cancer). They then compared what the AAPCC formula would have paid in capitation for beneficiaries in each of the cells with what their expected fee-for-service claims would have been, taking the health status variables into account. They found that the AAPCC formula resulted in overpayment of about $700 to $800 per person for the beneficiaries with the best health status, and underpayment of $1,700 to $1,800 for the sickest individuals.

The Mathematica/MCV researchers (Langwell and Hadley, 1989) analyzed actual data from TEFRA HMOs and reported in 1989 that favorable selection could have resulted in HCFA payments as much as 50 to 74 percent higher than the costs that

would have been incurred had the HMO enrollees been cared for in the fee-for-service sector. A more recent report by the same group, however, estimated the overpayment much more conservatively, at just 5.7 percent (Brown et al., 1993). The researchers also noted that although the capitation formula pays 95 percent of the projected fee-for-service costs, the actual costs incurred by TEFRA HMO enrollees were closer to 90 percent. Ellis and McGuire (1987) estimated that if strong favorable selection did indeed exist (i.e., if expected costs for HMO enrollees were one-third lower than costs for the average beneficiary in the fee-for-service sector), the capitation payment would only save money if it was reduced to 65 percent of the AAPCC.

In estimating the magnitude of the overpayment problem and its policy significance, it is important to consider the persistence of selection effects over time. If low users of health services join HMOs in disproportionate numbers and remain low users thereafter, then the HMOs will be overcompensated for many years. However, if low and high users of medical care regress to the mean over time, then incorrect payments are only a temporary problem. Unfortunately, a wealth of studies on this topic have failed to produce a clear answer as to whether regression to the mean occurs among Medicare beneficiaries. There are good studies which suggest that regression to the mean occurs and good studies showing that there are consistently high and low users of medical care. Welch (1985) argues in favor of regression to the mean. One dynamic at work, he notes, is that in the Medicare population, the sickest people disproportionately die. Additionally, the entire Medicare population ages over time and their health care needs increase. Consequently, HMO populations which may have been low users at enrollment cannot remain so forever. Welch also observed that no

more than 20 percent of an individual's health care costs can be predicted based on previous expenditures, which argues against the consistency of use hypothesis. A number of other researchers (Newhouse et al., 1981; Congressional Budget Office, 1982) have also found evidence of substantial regression to the mean for both the Medicare population and the non-elderly.

In contrast to these results, Anderson and Knickman (1984) found that high fee-for-service Medicare users in one year tend to remain high users in subsequent years (although Welch (1985) found in their data evidence of regression toward the mean). McCall and Wai (1983) showed that beneficiaries in the highest and lowest percentiles of use in one year tended to remain there the next year. Brown (1988) and Manton and Stallard (1992) noted that any regression effects due to the aging or mortality of the Medicare population could be partly or wholly countered by the adverse selection which has been found to occur in disenrollment from Medicare HMOs, as well as by favorable selection in new HMO enrollees.

Beebe (1988) reconciled these conflicting findings in part by suggesting that when beneficiaries are grouped on prior use, they will regress to the mean, but when they are grouped on the basis of demographic characteristics such as welfare status and age, reimbursement levels will be quite consistent over time. This is because the demographic groupings are more likely to cluster persons with chronic conditions together, while stratification on prior use tends to capture differences in reimbursement due to transitory, acute health events. It appears to be the consensus of most researchers that there is a permanent component of health which is predictive of about one-fifth of the variance in health care expenditures over time for a given individual

(Welch, 1985). This is substantial enough to make favorable selection effects a real policy concern and overpayment of Medicare HMOs an enduring problem.

The payment problem is thus a compelling reason to study the magnitude and source of selection bias in TEFRA HMOs. A second reason further study of this phenomenon is desirable is that selection effects have confounded research efforts to determine the impact of Medicare HMO cost-containment mechanisms on health care utilization. Because controlled trials in which beneficiaries are randomized to an HMO or a fee-for-service plan are a practical impossibility, studies of utilization in Medicare HMOs have been entirely nonexperimental. The noncomparability of the HMO and fee-for-service groups is a persistent source of concern which undermines the strength of the conclusions these studies have drawn about whether Medicare HMOs save money. It has been difficult to disentangle the cost-reducing effects of the HMO practice setting itself from effects due to favorable selection. Learning more about the nature of this favorable selection, and pursuing models of utilization which incorporate information about the simultaneity of the choice to participate in a Medicare HMO and subsequent utilization, would enable stronger conclusions to be drawn about the potential of managed care arrangements for reducing the costs of care for elderly populations.

2.4.2. Need for the present study

The present study builds and improves upon the existing literature on biased selection and utilization in Medicare HMOs in a number of ways. First, it uses more recent data and examines a wider range of variables than previous studies. There have

been seven studies which have evaluated data from Medicare HMOs from 1986 or later (Lichtenstein et al., 1991; Hill and Brown, 1990; Hill and Brown, 1992; Dowd et al, 1994; Riley, Lubitz, and Rabey, 1991; Price Waterhouse, 1996; Riley et al., 1996). Only Hill and Brown (1992), Price Waterhouse (1996) and Riley and colleagues (1996) examined several different dimensions of health status as possible indicators of selection bias. The present study uses data from 1993-1996 and seeks to validate and extend the findings of these studies by examining a number of sociodemographic and health status indicators of favorable selection.

Additionally, this study improves significantly upon the methodology of earlier studies. None of these studies have used panel data to examine the relationship between health status and HMO choice. Since an individual's health status and health care utilization may vary dramatically from year to year, it is important to collect data over a period of several years. This allows regression to the mean to be observed and unusual health events to be placed in the context of the individual's overall health. This study examines four years of data in order to form a more complete picture of beneficiaries' health history.

The methods used to examine utilization in this study are also more sophisticated than those employed in previous studies. Most previous research has failed to control sufficiently for selection bias into HMOs. It is difficult to separate utilization differences that are truly due to HMO cost-containment mechanisms from those that are due to differences in the health of HMO enrollees and non-enrollees.

This study uses both simultaneous equations and a discrete factor model to test the effect of Medicare HMOs on utilization, controlling for selection effects. Previous

studies have been unable to employ simultaneous equations due to the absence of variables for identifying the choice equation. However, in this study, identification is achieved through exclusion restrictions placed on variables measuring Medicare HMO market penetration, variables which have not been modeled before. The discrete factor model is a relatively new technique which has not previously been applied to study Medicare HMOs. It allows the effect of HMOs on utilization to be examined without concern for the normality of the disturbance.

Another contribution of this study is that the availability of data on market penetration makes possible analyses of differences in selection effects in HMOs operating in areas of low and high managed care penetration. None of the studies reviewed addressed this issue. This is a particularly interesting question in light of the large increases in beneficiary enrollment in HMOs, the increases in the number of risk contractors since the passage of TEFRA, and the changes which have occurred in the geographic distribution and market share of TEFRA HMOs throughout the United States.

This study provides a needed update to evaluations of the Medicare Capitation and Competition Demonstration projects. These studies, and even the studies from the late 1980s, may not provide an accurate picture of selection effects in TEFRA HMOs today for several reasons. The characteristics of HMOs choosing to enter into risk contracts with HCFA have changed since the early years of the risk program. Compared to the HMOs which participated in the demonstration projects, Medicare HMOs today are younger, smaller, serve smaller and less mature HMO markets, and receive less generous AAPCC reimbursement rates (Porell and Wallack, 1990). They

are more representative of the universe of HMOs in the United States than were the demonstration contractors, which were significantly different from non-participating HMOs. HMOs participating in the demonstrations were atypical in that they were much less likely than non-participating HMOs to be for profit, more likely to be staff model, and less likely to be IPA model (Langwell and Hadley, 1989). The proportion of Medicare HMOs that are IPA models has increased dramatically, and the proportion that are group, staff, or network models has dropped steadily, over the past ten years (Lamphere et al., 1997). Another change is that HMOs in the Capitation Demonstrations were permitted to perform health screening when enrolling elderly individuals, whereas TEFRA HMOs must enroll any interested beneficiary.

Demonstration HMOs offered very generous benefit packages with minimal copayments at very low premiums. Benefit packages have since become less extensive. Over the 1984-86 period, supplemental benefits such as vision, hearing, and dental services were reduced, and in most plans, cost sharing remained stable or increased (Langwell and Hadley, 1989). Seventy percent of the Competition Demonstration HMOs offered prescription drug benefits and hearing exams, but only 50 percent of TEFRA HMOs operating in 1995 did (United States House of Representatives, 1997). This number rebounded to 78 percent by 1997 (United States House of Representatives, 1997). The percentage of Medicare HMOs offering hearing aids and dental coverage has also declined since 1989. Langwell and Hadley (1989) speculated that demonstration HMOs may have been able to offer more generous benefits at a lower out-of-pocket cost to enrollees because they were operating in geographic areas with relatively higher AAPCC rates.

There have also been significant changes in the market penetration of Medicare HMOs. The Demonstration projects had to try to attract elderly beneficiaries who were unfamiliar with the concept of an HMO in markets with very low managed care penetration. Since the mid-1980s managed care has permeated the United States. Over 59 million Americans now receive their care from an HMO (American Association of Health Plans, 1996). With this increase in managed care market penetration and heightened awareness of the philosophy and functioning of managed care organizations has come expanded enrollment in Medicare HMOs. The increase in public awareness may have affected the likelihood that Medicare beneficiaries (and certain subgroups of beneficiaries) will be informed about the HMO option and will choose managed care. There is also a clear association between HMO penetration in an area's general health care market and the share of the Medicare market captured by TEFRA HMOs (Welch, 1996).

Since the data used in the Mathematica/MCV evaluation were gathered, the share of the Medicare market captured by TEFRA HMOs has risen from 3.5 percent in 1985 to 7.1 percent in 1993 to 11.2 percent in 1996 (Health Care Financing Administration, 1996a). Moreover, risk contracting has spread far beyond the 12 market areas of the Competition Demonstrations, and enrollees have been spread among a greater number of contractors. These changes in TEFRA managed care penetration raise a question as to whether the composition of the HMO member population has changed over time. Any or all of the changes in the characteristics of Medicare risk contractors, their marketing strategies, the benefit packages offered, the premiums charged, public knowledge about managed care, managed care penetration in

the general population, and the expansion and geographic dispersion of Medicare risk contractors may have affected the relative attractiveness of Medicare HMOs to various groups of Medicare beneficiaries. Additional study of the composition of the enrolled population is consequently desirable.

Finally, this study's focus on HMO market penetration represents a significant innovation on the existing literature on HMO enrollment dynamics. The possible effect of increasing HMO market penetration on the extent of biased selection in HMOs has not previously been examined in empirical studies. A few researchers have speculated that favorable selection will weaken over time as enrollment in Medicare HMOs increases. Ellis and McGuire (1987) theorized that the expected cost per HMO enrollee would increase with Medicare HMO market share in a linear or near-linear fashion. Rossiter and colleagues (1989) also predicted that favorable selection would diminish as Medicare HMO enrollment expanded "because the marginal enrollee is more likely to be like the average fee-for-service beneficiary with increasing market penetration." The Health Care Financing Administration, Office of Management and Budget, and Department of Health and Human Services also believed at the time of the Competition Demonstrations that favorable selection would disappear when enrollment in the risk program jumped under TEFRA (Rossiter, Friedlob, and Langwell, 1985).

These hypotheses have not yet been empirically tested; existing work is purely speculative in nature. However, the issue strongly lends itself to empirical study because there are very wide variations in Medicare HMO market penetration between market areas in the United States. For example, the Riverside/San Bernadino, California Metropolitan Statistical Area (MSA) had 47.3 percent Medicare HMO

market share in 1994, while other MSAs such as New Orleans and Detroit had negligible market shares (Welch, 1996). This wide range of values for the independent variable makes it likely that a regression analysis with market areas as the unit of analysis will reveal interesting information about the relationship, if any, that market penetration bears to selection effects. This study makes a substantial contribution in this area by proposing a theoretical relationship between market penetration and favorable selection and testing it using empirical data.

3. Theory and conceptual model

3.1. Medicare HMO choice

3.1.1. Overview of consumer choice theory

This study is grounded in the traditional economic theory of consumer choice. Enrollment in a Medicare HMO is conceptualized as a voluntary choice made by Medicare beneficiaries on the basis of a comparison of the costs, risks, and benefits of HMO membership versus remaining in a fee-for-service arrangement. Consumers will choose the affordable bundles of goods and services which maximizes their utility. The affordability of bundles is determined by the consumer's income and the prices at which goods can be bought, while the extent to which a particular bundle maximizes utility depends on the consumer's tastes and preferences. This theory, also called expected utility maximization theory, has been used extensively in the literature to analyze health plan choice by employed persons (Friedman and Savage, 1948; Friedman, 1974; Feldman, Dowd, Finch and Cassou, 1989; Lairson and Herd, 1987) and the demand for health insurance generally (Arrow, 1971; Borch, 1968; Phelps, 1976).

Consumer choice theory rests on four assumptions. First, it assumes that individuals are rational utility maximizers. Second, it treats health plans as a normal economic good. Third, it recognizes that substantial inertia exists in health plan choice and that there are transaction costs associated with switching, so that a consumer must perceive a significant advantage in order to switch plans (Lairson and Herd, 1987;

Schuttinga, Falik, and Steinwald, 1985). Neipp and Zeckhauser (1985) have shown that 95 percent of individuals remain in their chosen health plan each year. Fourth, it assumes that consumers have adequate information to make rational choices between health plans.

Though consumer choice theory is deeply engrained in classical health economics, it is worthwhile to examine critiques of the theory relating to these assumptions. There is a considerable literature on the subject of whether individuals truly act as rational utility maximizers. Rice (1997) observes that consumers may have tastes that do not actually maximize their utility. For example, with regard to health plan tastes, they may prefer to stay in their current plan simply because it is what they are used to, regardless of the fact that an "objective" analysis would reveal that another plan would serve their needs better. Rice argues that consumer preferences exhibit "a strong advantage for whatever is the status quo; familiarity breeds preference," and that a competitive marketplace in which consumers act on their preferences may not actually make people better off.

Another problem is that consumers may not, in fact, have adequate information to form rational preferences. This seems particularly true with regard to the choice between health plans. Some authors contend that Medicare beneficiaries generally do not receive enough comprehensible information about the differences between their health care options and demonstrate low levels of knowledge about the coverage offered by Medicare and supplemental policies (Davidson, 1988). In general, consumers do not appear to devote much effort to searching for information about their health care options (Rice, 1997; Hoerger and Howard, 1995). They consequently may

not have sufficient information to identify the health plan option which would maximize their utility.

Even if consumers do have adequate information, they may encounter cognitive difficulties in processing the information. Kahneman (1994), Kahneman and Tversky (1979), and Rice (1997) identify several problems which cause people to form preferences that systematically violate the axioms of expected utility theory. Choice experiments suggest that decision utilities may be extremely myopic, focusing on the utility associated with transactions rather than the utility of long-term outcomes (Kahneman, 1994). People may also tend to make decisions about future consumption without due consideration of possible future changes in their tastes (Kahneman, 1994; Simonson, 1990). Fallible memory and incorrect evaluation of past experiences may lead to suboptimal choices for the future (Kahneman, 1994). Additionally, consumers may not be able to predict the results of their choices (Rice, 1997). This seems particularly true of health care choices, since one cannot usually predict whether medical treatments will be successful or not (Weisbrod, 1978). This could also lead to decisions that ultimately fail to maximize utility. Kahneman and Tversky (1979) describe a tendency for decision-makers to overweight outcomes that are considered certain, relative to outcomes which are merely probable. They also note that the framing of the choice problem has been shown to affect the decisions people make. In particular, people tend to make different choices when the options are framed in terms of gains than when they are described in terms of losses. They tend to display risk aversion in the domain of gains and risk seeking in the domain of losses. Finally, in order to simplify choices, people often disregard components that the alternatives have

in common, and focus on the ways in which they differ. This can lead to inconsistent preferences "because a pair of prospects can be decomposed into common and distinctive components in more than one way, and different decompositions sometimes lead to different preferences" (Kahneman and Tversky, 1979). For all of these reasons, consumers may not act as rational utility maximizers in making health plan choices, and the assumptions of consumer choice theory may not be satisfied. People do not always conform to the dictates of economic theory, and economic models can only go so far in predicting human behavior.

Having said this, consumer choice theory has still been found to be very useful for examining the choice of health plans by Medicare beneficiaries. A particular strand of consumer choice theory called the risk vulnerability hypothesis (Bashshur and Metzner, 1967, 1970) has dominated the health plan choice literature since the late 1960s. This hypothesis posits that individuals who have greater perceived health risk or greater economic risk because of scant personal financial resources will be more likely than individuals with lower health and economic risk to choose an HMO plan, with its broader benefits and lower out-of-pocket costs. Consequently, the hypothesis predicts, HMOs will experience adverse selection.

The risk vulnerability hypothesis was modified in the late 1970s by Bice (1975) and Berki and Ashcraft (1977, 1980) after earlier studies failed to produce consistent empirical support for the model and its prediction of adverse selection. Berki and Ashcraft disaggregated the risk vulnerability hypothesis into a hypothesis concerning health risk and a hypothesis concerning financial loss. Their risk perception hypothesis states that the higher the probability an individual attaches to future events requiring

medical care, the more likely she is to enroll in a plan which, *ceteris paribus*, offers the more comprehensive, accessible benefit package. An individual's risk perception is affected by their perceived heath status, level of health concern, previous utilization, and degree of risk aversion. The financial loss hypothesis states that the larger the expected utility loss associated with a given expected financial loss, the more likely the individual is to enroll in a plan which, *ceteris paribus*, reduces the financial costs of utilization. The utility loss associated with a financial loss depends on the individual's income and wealth and degree of risk aversion.

Berki and Ashcraft hypothesized that individuals with *both* high perceived health risk *and* economic vulnerability will be more likely than healthier, less economically vulnerable persons to choose the HMO plan. However, if both of these conditions are not present, perceived health risk and economic vulnerability by themselves will have little impact on health plan choice. Thus, the risk perception/financial loss hypothesis predicts adverse selection into HMOs only for certain types of individuals (Berki and Ashcraft, 1980). Bice (1975) and Berki and Ashcraft (1980) also noted that the financial loss hypothesis may have little relevance for individuals covered by public insurance programs such as Medicare, since the economic losses for which they are at risk are relatively small.

The Berki and Ashcraft model has been widely used in studies of health plan choice. Empirical support for the model has been mixed; the risk perception and financial loss hypotheses have been neither confirmed nor disproven (Sofaer and Hurwicz, 1993). The Berki and Ashcraft model is used as a theoretical starting point in the present study, but major alterations to the theory are made. The theory is

applied specifically to Medicare beneficiaries who, as noted above, are covered for many if not most high-cost health services regardless of their plan choice. Bice and Berki and Ashcraft's assumption that strong relationships exist between perceived health risk, economic vulnerability, and health plan choice is retained in this study, but it is theorized that these factors interact to produce *favorable* rather than adverse selection into HMOs. This will be explained in greater depth shortly.

3.1.2. Econometric specification of the theory

Consumer choice theory is traditionally operationalized through the construction of a random utility model. This model, advanced by Luce and Suppes (1965) and others and placed in an econometric framework by McFadden (1973), defines utilities to the consumer of two different choices as follows:

U_{i1} = expected utility that individual i derives from alternative 1

U_{i2} = expected utility that individual i derives from alternative 2

Let alternative 1 be a Medicare HMO and alternative 2 be a fee-for-service plan. If we observe individual i choosing to join an HMO, then we know that she prefers the HMO to fee-for-service. That is, she derives greater expected utility from the HMO choice than she does from fee-for-service. We may define an unobservable variable y_i^* which represents the difference in the utilities the individual derives from the two choices:

$$y_i^* = U_{i1} - U_{i2}$$

If we observe the individual choosing the HMO, we know that $U_{i1} > U_{i2}$, so $y_i^* > 0$. On the other hand, if the individual chooses fee-for-service, then we know that $U_{i1} <$

U_{i2}, so $y_i^* < 0$. We can then specify a regression model in which y_i^* is a function of a vector **X** of income, price, and tastes variables:

$$y_i^* = \mathbf{bX}_i - u_i$$

However, since y_i^* is not observed, we must define a dummy variable y_i which takes on a value of one if $y_i^* > 0$ (if the person chooses a Medicare HMO) and a value of zero otherwise. Then we can model HMO choice, using an appropriate binary choice estimation method, as:

$$\Pr(y_i = 1) = \Pr(y_i^* > 0) = \Pr(u_i < \mathbf{bX}_i) = F(\mathbf{bX}_i),$$

where F is the cumulative distribution function for u_i. Estimation methods are discussed in depth in Section 4.

3.1.3. Applications and extensions of consumer choice theory in the present study

Following consumer choice theory, this study conceives of enrollment in a Medicare HMO as a voluntary choice made by Medicare beneficiaries on the basis of a comparison of the costs, risks, and benefits of HMO membership relative to remaining in a fee-for-service plan. A consumer's choice in this regard is theorized to be affected by both the *personal characteristics of the decision-maker* and the *characteristics of the local Medicare health plan market*. Before discussing the influence of these two sets of factors, it is useful to outline briefly the differences in cost and access to services involved in selecting an HMO over a traditional Medicare plan.

A Medicare beneficiary must decide between three health plan options: Medicare HMO, Medicare fee-for-service with a Medigap supplementary insurance plan, and Medicare fee-for-service without Medigap. The HMO and Medigap options

are substitute products in the sense that they are purchased in order to obtain roughly similar kinds of supplementary benefits. The analysis which follows frames the choice as binary (joining an HMO or not joining an HMO), but in fact the non-HMO choice encompasses two possible alternatives: fee-for-service without a Medigap policy and fee-for-service with Medigap. About three-fourths of Medicare beneficiaries have some kind of Medigap policy, and thus most beneficiaries themselves will view the choice in terms of Medigap versus HMO.

A consumer choosing between these health plan options must consider the ways in which out-of-pocket costs and access to services might differ in HMO and fee-for-service plans. The relative cost of these services is the difference in the total out-of-pocket cost that an individual using a given amount of these services in the two alternative plans would incur, including all deductibles, copayments, and other out-of-pocket costs (but assuming for the moment a zero HMO premium). Although the price of basic Medicare coverage is the same for beneficiaries whether they choose to participate in an HMO or remain in a traditional arrangement, participation in an HMO reduces the enrollee's cost for certain goods and services not normally covered by Medicare, such as preventive care and prescription drugs. As was noted earlier, HMOs are usually able to provide these services at a lower cost to the subscriber than traditional Medigap supplementary insurance policies. However, membership in an HMO may at the same time increase the cost of other kinds of goods and services, such as specialist care and diagnostic tests, which may be more difficult to access in an HMO than in a fee-for-service plan.

The calculation of the marginal out-of-pocket cost of an HMO plan relative to a fee-for-service plan is complicated by the existence of HMO premiums. If the HMO charges a monthly premium, the cost calculation must include this out-of-pocket cost, which must be paid up-front and regardless of use. If an HMO enrollee does not use enough health services to recoup the money paid in premiums, HMO membership may actually result in higher out-of-pocket costs than the beneficiary would have incurred in a basic fee-for-service plan (though it would still probably be less than she would have paid in a fee-for-service arrangement with a traditional Medigap policy). Traditional economic theory dictates that demand for a particular health insurance policy will vary inversely with the portion of the premium that exceeds the actuarial value of anticipated losses (Broyles and Rosko, 1988).

The ease with which health care services may be accessed may differ in HMOs and fee-for-service plans because of the commitment of HMOs to reducing "inappropriate" utilization. Physician gatekeepers may restrict access to specialist care and expensive tests and procedures, and prospective or concurrent utilization review may result in denial of coverage for procedures or other care deemed unnecessary. Of course, HMO membership may also result in increased access to some services because of the more comprehensive, first-dollar coverage provided.

With this understanding of the concepts of relative cost and access, we can compare the relative costs of and access to routine, acute, and chronic care services in HMOs and fee-for-service plans. It is fairly clear that a Medicare HMO will provide more comprehensive coverage for routine services such as preventive care, eye and ear care, dental services, physical exams, and immunizations than basic fee-for-service

plans without Medigap, and consequently the out-of-pocket costs to patients who use these services would be lower in an HMO than in fee-for-service. Medigap supplemental insurance plans may provide coverage for these services, but typically cover only a few basic preventive services. The major difference in out-of-pocket costs between HMO and Medigap fee-for-service plans is likely to be the premium amounts. Access to these services is likely to be better in HMOs than in basic fee-for-service plans because there are fewer financial barriers (in the form of out-of-pocket costs) to be overcome in seeking care.

With regard to acute care services (i.e., inpatient and outpatient services for injuries and illnesses of short duration), there may be a considerable difference in the out-of-pocket costs users will accrue in a Medicare HMO plan versus a fee-for-service plan with Medigap if the HMO offers a prescription drug benefit with little or no copayment. Compared to a fee-for-service plan without Medigap, a zero-premium HMO plan would definitely provide acute care services at a lower cost to the subscriber. The HMO plan would provide better coverage for physician office visits and prescription drugs and cover the deductibles and copayments for which a Medicare beneficiary is otherwise liable. However, since basic Medicare does cover most of the expenses associated with acute care services, the magnitude of the savings from HMO membership may not be very great. Moreover, HMO membership may entail greater out-of-pocket costs for acute care services if the HMO, in its gatekeeping and utilization review functions, determines that some diagnostic test, procedure, or specialist referral desired by the patient or her physician is not appropriate and will not be covered. A patient who still feels that these services are necessary would have to

pay for them out-of-pocket. Thus, while the HMO would seem to offer a lower marginal cost for acute care services, the difference may be small and uncertain.

A similar analysis applies to chronic care services. HMOs generally provide more comprehensive, first-dollar coverage, but may also entail greater risk of restricted access to desired services. Even more importantly, HMOs of the closed-panel variety may place stringent restrictions on access to preferred physicians. Enrollees of closed-panel HMOs are only covered for services rendered by a physician who has a contract with the HMO. This consideration has particular salience for chronic care services because chronically ill individuals are likely to have an established relationship with a physician which they will be reluctant to terminate in order to join a closed-panel HMO. This "physician tie" issue is a critically important factor in health plan choice. The maintenance of an existing physician relationship has been found to be highly influential in an individual's decision to remain in a fee-for-service plan (Luft, 1980; Berki and Ashcraft, 1980; Juba, Lave and Shaddy, 1980). For users of chronic care services, HMO membership may entail substantial access limitations and only small savings in out-of-pocket costs.

3.1.4. Effect of individual characteristics on health plan choice

How do Medicare beneficiaries use this information about relative costs and ease of access to make health plan choice decisions? It is theorized that each beneficiary attaches a different degree of importance to the differences in cost and access based on three personal characteristics: (1) their perceived likelihood of needing or wanting to use each of these types of services; (2) their attitudes towards risk; and

(3) their personal financial resources. Of these, perceived need for services is probably the most important. Perceived need for services is affected by the individual's perceived health status, which provides him/her with information about the nature, frequency, and intensity of health services she is likely to require in the future.

Individuals who perceive themselves to be in good health are likely to value cost savings over unrestricted access to physicians and services. They will also value routine health care services very highly, because this is the category of services they expect to use the most. They are interested in maintaining their good health through preventive care and wellness programs, eye and ear exams, immunizations, and other routine services, and they are interested in securing these services at the lowest possible out-of-pocket cost. Because out-of-pocket costs are generally lower in HMOs than in fee-for-service plans, and coverage for routine services more generous, healthier individuals will tend to choose HMO plans over fee-for-service plans. In contrast, individuals in relatively poor health will forgo the cost savings possible in an HMO in order to preserve their access to a preferred physician and to expensive acute and chronic care services. They are more likely than healthier individuals to have a prior attachment to a particular physician, and a prior physician relationship is "the single largest and most consistent" predictor of consumer retention in a plan that covers that physician's services (Mechanic, 1989). Thus, the chronically ill are expected to have a very strong preference for remaining in a fee-for-service setting.

These preferences may be strengthened or attenuated by an individual's attitudes about economic risk. A high degree of aversion to the risk of incurring out-of-pocket costs will augment the importance of cost considerations (which favor the HMO)

relative to access to care considerations (which favor the fee-for-service plan). Conversely, a risk-seeking individual will place greater emphasis on the opportunities for easy access to services in a fee-for-service plan, and be more willing to risk incurring the out-of-pocket expenses that may be associated with such plans.

Health plan preferences are further affected by an individual's personal financial resources, which affect the degree of importance she attaches to differences in out-of-pocket costs between HMO and fee-for-service plans. Persons with low income and wealth will have difficulty meeting out-of-pocket health care costs, whereas higher income persons may not. This will tend to increase the salience of economic considerations relative to access to care considerations for low income persons, and diminish the importance of cost differences for high income persons. Regardless of self-perceived need for services, having a low income will make beneficiaries less disposed to choose fee-for-service and more disposed to choose an HMO, though high need individuals will still have a weak preference for fee-for-service.

To summarize the foregoing, perceived need for health services, attitudes towards risk, and personal financial resources predispose individuals to choose health plans as described in Table 3.1.

Table 3.1. Theoretical Effect of Individual Characteristics on Health Plan Choice

	Decision-maker characteristics							
	Low perceived need (good health)				High perceived need (poor health)			
	Low aversion to risk of out-of-pocket costs		High aversion to risk of out-of-pocket costs		Low aversion to risk of out-of-pocket costs		High aversion to risk of out-of-pocket costs	
	Low income	High income	Low income	High income	Low income	High income	Low income	High income
Choice:	++	+	+++	++	--	---	-	--

\+ = weak preference for HMO
++ = moderate preference for HMO
+++ = strong preference for HMO
\- = weak preference for fee-for-service
-- = moderate preference for fee-for-service
--- = strong preference for fee-for-service

In this Table, the cells represent the strength of an individual's preference for the HMO plan relative to the fee-for-service plan. The pattern displayed in the Table is one of favorable selection into HMOs. Individuals with lower perceived need for services (i.e., better health) disproportionately choose the HMO, while the chronically ill tend to prefer the fee-for-service plan. A high degree of risk aversion will cause an individual to look more favorably upon the HMO option, with its decreased risk of out-of-pocket expenditures. Having scant personal financial resources also increases the attractiveness of the HMO choice. Because low income is associated with both relatively low utilization (Leopold and Langwell, 1978; Garfinkel, Riley and Iannacchione, 1988) and a higher likelihood of choosing an HMO, the influence of personal financial resources on health plan choice tends to exacerbate favorable selection in HMO enrollment. In summary, the formulation of consumer choice theory used in this study predicts that rational utility calculations by Medicare beneficiaries will result in favorable selection into HMOs.

If the above theory has merit, one would expect to find empirical support for the proposition that there is a negative relationship between perceived need for services and Medicare HMO choice and the proposition that there is a negative association between income and Medicare HMO choice. There is ample evidence for the first proposition. Many studies have found significant negative associations between perceived health status and the likelihood that a person will choose an HMO plan, whether the specific measure used is general self-perceived health (Brown, 1988; Kasper et al., 1988), functional impairment (Brown, 1988; Retchin et al., 1992; Kasper et al., 1988; Lichtenstein et al., 1991; Hill and Brown, 1992), or presence of chronic health conditions (Garfinkel et al., 1986; Juba, Lave, and Shaddy, 1980; Langwell and Hadley, 1989; Riley et al., 1996).

The second proposition finds considerably less support in the existing literature. Studies have found both a negative (Brown and Langwell, 1988) and a positive (Rossiter et al., 1989; Dowd et al., 1994) relationship between income and the probability of choosing a Medicare HMO. Studies of non-Medicare HMOs have provided more consistent support for the hypothesized negative relationship between income and HMO choice (Scitovsky, McCall, and Benham, 1978; Juba, Lave, and Shaddy, 1980; Grazier et al., 1986; Buchanan and Cretin, 1986; Welch and Frank, 1986). It is hoped that the present study will provide additional empirical support for the theorized income effect.

3.1.5. Effect of market area characteristics on health plan choice

Thus far the effect of individual characteristics on health plan choice has been discussed. But health plan choices are also theorized to be affected by *characteristics of the health plan market* in which the decision-maker is operating. Among the relevant market characteristics are the *HMO plan options* available to Medicare beneficiaries and the degree of *market penetration* achieved by HMOs in the area. Medicare beneficiaries may be more disposed to join an HMO if they have the option of enrolling in an HMO with desirable characteristics such as an open-panel form (which allows members to maintain existing physician relationships), a good prescription drug benefit, or a zero premium. The HMO options available in a market are closely related to the extent of HMO market penetration in that high-penetration markets are more likely than low-penetration markets to have two or more HMOs operating in them, each offering one or more different types of plans.

HMO market penetration also affects beneficiaries' health plan choices directly in several ways. First, Medicare beneficiaries residing in areas where a significant percentage of the population is cared for by HMOs are likely to be familiar with managed care as a concept. They are more likely to be aware of their opportunity to join an HMO, and less likely to remain in fee-for-service out of a "fear of the unknown," than individuals in low-penetration areas.

Second, physicians become more disposed to contract with HMOs as managed care penetration increases. It is reasonable to assume that if a physician enters into an exclusive contract with an HMO in which she agrees to see only HMO patients, the physicians' former patients will likely join the HMO too in order to maintain their

established physician relationship. This seems especially true for Medicare patients since HMO membership is likely to cost them less than a fee-for-service plan. If a physician enters into a non-exclusive contract with a Medicare HMO, the patient would still have access to the physician even if they chose not to join the HMO. However, since they could maintain their physician tie as an HMO member, a very significant obstacle to joining an HMO would have been overcome. Thus, an increasing number of physician contracts with Medicare HMOs – whether they are exclusive contracts or not – will predispose a larger proportion of Medicare patients to join HMOs. Since sicker patients are more likely than healthier individuals to have strong physician ties, this dynamic will primarily have the effect of encouraging more persons in relatively poor health to switch to an HMO.

There is evidence that as managed care penetration (as measured by the percent of persons in a given market who are enrolled in an HMO or PPO) increases, so does the percentage of physicians in the market who enter into contracts with managed care organizations. As more and more patients in a given market become enrolled in managed care plans, physicians feel increasing economic pressure to contract with these plans in order to maintain or build their patient base (Rosenbach, Harrow and Hurdle, 1988). The last ten years have seen significant national increases in both managed care penetration and physician participation. The percentage of Americans enrolled in a managed care plan has increased from 27 percent in 1987 to 75 percent in 1997 (Hoy et al., 1991; Emmons and Wozniak, 1997). The percentage of physicians with managed care contracts increased from 61 percent in 1988 to 88 percent in 1996 (Emmons and Wozniak, 1997; Emmons and Simon, 1994, 1996). The average physician now holds

contracts with eight different managed care plans (Collins and Sandman, 1996). Fifty-six percent of physicians in the United States now have contracts with Medicare HMOs (Emmons and Wozniak, 1997). Details of the increase in physician participation in managed care are presented in Table 3.2.

Table 3.2.
Percentage of Physicians with Managed Care Contracts, 1988-1996

Type of Contract	1988	1989	1990	1991	1992	1993	1994	1995	1996
IPA	21	19	19	20	21	27	32	39	50
HMO	35	33	36	37	42	49	55	64	72
PPO	45	45	49	54	58	66	64	69	75
Any	61	59	61	65	70	76	77	83	88

Source: Emmons and Wozniak, 1997; Emmons and Simon, 1994, 1996

In addition to this national temporal correlation between managed care penetration and managed care contracting, there is some evidence of geographic correlations. Metropolitan areas are much more likely to have high levels of managed care penetration than rural areas, and physicians in large metropolitan areas are much more likely to participate in managed care (Emmons and Simon, 1994). Additionally, census regions with relatively high penetration of Medicare HMOs also tend to exhibit relatively high levels of physician contracting with Medicare HMOs, and vice-versa for regions with relatively low penetration. For example, Table 3.3 shows that though the pattern is not completely consistent across all census regions, both penetration and physician participation are high in the Pacific and Mountain regions, and both are low in the East North Central, South Atlantic, and East South Central regions.

Table 3.3. Medicare HMO Penetration and Physician Participation in 1996, by Census Region

Census Region	Medicare HMO Penetration (% of beneficiaries in HMOs)		Percentage of Physicians with Medicare HMO Contracts	
Pacific	**HIGH:**		**HIGH:**	62.6%
	CA , OR, HI	≥25%		
	WA	5-15%		
	AK	0 %		
Mountain	**HIGH:**		**HIGH:**	59.5%
	AZ, NV	≥25%		
	NM, CO, UT	5-15%		
	ID, WY, MT	0 %		
West North Central	**MEDIUM:**		**HIGH:**	61.1%
	MN	16-25%		
	MO	5-15%		
	ND, NE, KS, IA	<5%		
	SD	0%		
Mid-Atlantic	**MEDIUM:**		**MEDIUM-HIGH:**	58.0%
	PA, NY	5-15%		
	NJ	<5%		
New England	**MEDIUM-LOW:**		**HIGH:**	59.6%
	MA, RI	5-15%		
	VT, CT	<5%		
	ME, NH	0%		
West South Central	**MEDIUM-LOW:**		**MEDIUM-HIGH:**	57.1%
	TX, LA	5-15%		
	OK	<5%		
	AR	0%		
South Atlantic	**MEDIUM-LOW:**		**LOW:**	50.7%
	FL	16-25%		
	GA, NC, VA, WV, MD	<5%		
	DE, SC	0%		

East North Central	LOW:		LOW:	51.0%
	IL	5-15%		
	WI, MI, IN, OH	<5%		
East South Central	LOW:		LOW:	52.4%
	AL, KY	<5%		
	MS, TN	0%		

Source: Gonzalez, 1997; Health Care Financing Administration, 1996

Thus, physicians practicing in areas with high managed care penetration are more likely to hold contracts with managed care plans than are physicians in areas of low penetration. It follows that patients residing in high-penetration areas who have strong ties to a particular physician are less likely to find that joining a Medicare HMO will mean that they can no longer see their regular physician. Since chronically ill patients are particularly likely to have strong physician ties, this suggests that chronically ill patients in high-penetration areas will face fewer obstacles to joining a Medicare HMO than patients in low-penetration areas. The effect of managed care penetration is consequently to weaken favorable selection into Medicare HMOs.

A third reason that favorable selection may weaken as managed care penetration increases relates to the marketing strategies of HMOs. Medicare HMOs have a financial incentive to focus their marketing and recruitment efforts on the healthiest available stratum of the Medicare population. In a low-penetration market, HMOs can seek to recruit the very healthiest segment of the market. For the reasons discussed earlier, these healthy individuals are more likely than sicker persons to find that the benefits of joining an HMO outweigh the risks and costs, so the HMOs are likely to be

successful in disproportionately enrolling low-risk individuals. Thus, favorable selection is expected in markets with low HMO penetration.

There are, however, a limited number of very low-risk Medicare beneficiaries in any given market area, and if the HMO wishes to expand its enrollment it must at some point reach into higher-risk strata. As the HMO enrolls a greater and greater percentage of the Medicare population, it will expand its marketing efforts to higher-risk individuals. These marketing efforts, combined with the greater awareness of and experience with HMOs in the community which accompanies increasing HMO market share, will make higher-risk persons more disposed to join a Medicare HMO as market penetration increases. Favorable selection can therefore be expected to diminish with increasing HMO market penetration. There is some question, however, as to the threshold level of market penetration at which this effect will be observable. It is possible that current levels of Medicare HMO penetration (around 13 percent) are below that threshold.

The relationship between HMO market penetration and favorable selection has not been extensively discussed in the literature to date. However, a few researchers have speculated that favorable selection will weaken over time. Ellis and McGuire (1987) theorized that the expected cost per HMO enrollee would increase with Medicare HMO market share in a linear or near-linear fashion. They did not place a ceiling on HMO market penetration, but rather extended the cost function out all the way to 100 percent HMO market share. Rossiter et al. (1989) also predicted that favorable selection would diminish as Medicare HMO enrollment expanded "because the marginal enrollee is more likely to be like the average fee-for-service beneficiary

with increasing market penetration." The Health Care Financing Administration, Office of Management and Budget, and Department of Health and Human Services also believed at the time of the Competition Demonstrations that favorable selection would disappear when enrollment in the risk program jumped under TEFRA (Rossiter, Friedlob, and Langwell, 1985).

No studies were identified which have examined differences in the degree of favorable selection between market areas with different levels of Medicare HMO penetration using empirical data. Existing work is purely speculative in nature. However, this issue strongly lends itself to empirical study because there are very wide variations in Medicare HMO market penetration between market areas in the United States. For example, the Riverside/San Bernadino, California Metropolitan Statistical Area (MSA) had 47.3 percent Medicare HMO market share in 1994, while other MSAs such as New Orleans and Detroit had negligible market shares (Welch, 1996). This wide range of values for the independent variable makes it likely that a regression analysis with market areas as the unit of analysis will reveal interesting information about the relationship, if any, that market penetration bears to selection effects.

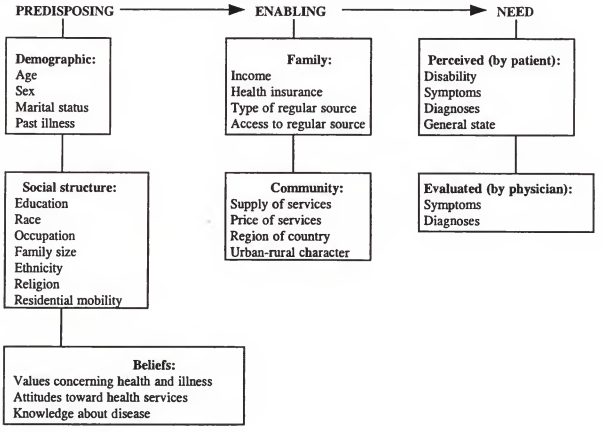
3.2. Health services utilization by the elderly

Because this study will also examine the effect of Medicare HMO cost-containment mechanisms on health services utilization, it is important to understand the factors which affect health care use by elderly individuals. The behavioral model of utilization developed by Andersen and colleagues (Andersen, 1968; Andersen and Newman, 1973; Aday and Andersen, 1974, 1975; Aday, Andersen, and Fleming,

1980) provides a comprehensive framework for explaining and predicting health services use. The behavioral model is "the most widely used conceptual framework in the field" of utilization research (Wolinsky and Johnson, 1991) and has been applied specifically to elderly individuals by Wolinsky and colleagues (Wolinsky et al., 1983; Wolinsky and Coe, 1984; Wolinsky, Mosely, and Coe, 1986; Wolinsky and Johnson, 1991; Wolinsky, 1994).

The behavioral model states that the use of health services is a function of certain predisposing, enabling, and need characteristics of the individual. These characteristics are set forth in Figure 3.1:

Figure 3.1. The Behavioral Model of Health Services Utilization



The predisposing characteristics in the model reflect the fact that some individuals have a greater propensity to use services than do other individuals. Andersen theorizes that these propensities can be predicted by analysis of the demographic, social-structural, and health beliefs characteristics listed in Figure 3.1. The model asserts that individuals with different demographic characteristics have different types and amounts of illness and that individuals with different social-structural characteristics have different lifestyles, both of which impact the individuals' health services utilization. Moreover, these patterns will vary with the salience of health beliefs.

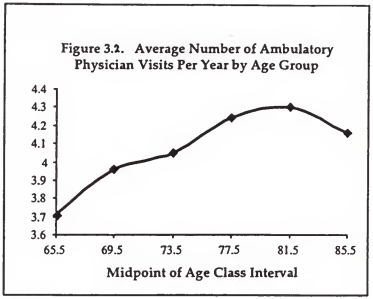
The enabling characteristics reflect the fact that regardless of his predisposition to use health services, an individual will not actually use them unless able to do so. Access to services is a function of the family and community characteristics described in Figure 3.1. Essentially, this portion of the model states that individuals will not be able to use health services unless their personal financial resources are adequate to afford the services and the services are in ready supply in the community.

The predisposing and enabling characteristics are necessary but not sufficient conditions for the utilization of health services. Individuals must also perceive some medical need for using health services. Need may be based on the individual's own perceptions of his health status, or it may derive from a health care provider's assessment of the individual's medical condition. Need is the immediate and direct trigger which leads to the utilization of health services; it puts the predisposing and enabling characteristics into action.

Although the behavioral model was originally developed to explain utilization by families, it has also been extensively applied to elderly individuals (Wan, 1989; Wolinsky et al., 1983; Wolinsky and Coe, 1984; Wolinsky, Mosely, and Coe, 1986; Wolinsky and Johnson, 1991; Wolinsky, 1994). Studies of utilization by Medicare beneficiaries have produced a substantial amount of empirical evidence for the relationships hypothesized in the behavioral model. Two comprehensive literature reviews by Wan (1989) and Epstein and Cumella (1988) determined that need variables, including perceived health, chronic conditions, and functional limitations, have consistently been found to be highly statistically significant predictors of both hospital and physician utilization. Enabling variables have exhibited a less consistent performance across studies. A significant effect for regular source of care has been observed in most studies, while effects for private insurance coverage and income have been observed in about half of the published studies (Wan, 1989). Community-level enabling variables have not often been tested, but Welch (1991) found that population density, population size of the metropolitan area, and rural location jointly explained more than 60 percent of the observed variation in Medicare expenditures in his sample. Welch also determined that supply characteristics such as hospital beds, primary care physicians, and specialists per thousand persons were statistically significant predictors of Medicare expenditures. Considerable variation in study findings has been reported for predisposing variables. Wan (1989) reported that age and sex have achieved statistical significance in a majority of studies, but education, marital status, and employment status frequently have not. Epstein and Cumella's (1988) review found sociodemographic characteristics to be significant predictors of utilization in 44 percent

of the 419 instances examined. Age was significant in 60 percent instances examined, sex in 46 percent, race in 63 percent, income in 51 percent, and education in just 22 percent of instances.

The effect of age on utilization is complex and worthy of special examination. Research findings on the nature of this relationship have varied, but most have agreed that the relationship is not strictly linear. Studies of the elderly which have used an age-stratification approach rather than treating age as a continuous variable have found different age effects for the "young old" (approximately age 65 to 79) and the "old old" (roughly age 80 and above). Garnick and Short (1985) showed that very old patients tended to have greater severity of illness when admitted to the hospital and consequently had longer stays and incurred greater costs than patients age 65 to 79. Wolinsky, Mosely and Coe (1986) stratified their sample of 64- to 87-year-olds into six class intervals and observed an "inverse J-curve" aging effect in which utilization (as measured by number of physician visits per year) rises with age until around age 80, then levels off and decreases to age 87, as shown in Figure 3.2:



Riley and colleagues' (1987) study of Medicare reimbursements, which used expenses rather than number of visits as the dependent variable, found that this inverse J-curve relationship held for patients who died during the year studied but that for survivors, reimbursements were higher for patients 85 years and older than for patients aged 65 to 74. The precise nature of the age effect is thus a subject of controversy, but there is general agreement that modeling age as a continuous variable obscures differences in effect for the "young old" and the "old old."

Overall, the behavioral model has been found to explain up to one quarter of the variation in health care utilization by Medicare beneficiaries (Wolinsky and Coe, 1984; Wan, 1989). Although this means that a substantial proportion of the variation remains unexplained by the model, researchers have generally concluded that, regardless of the underlying model used, the maximum achievable R-squared in models of health care utilization by Medicare beneficiaries is around 0.20 (McCall and Wai, 1983; Welch, 1985; Newhouse et al., 1989; van Vliet and van de Ven, 1993). It is interesting to note

that the current AAPCC payment formula has generally been found to explain only about 2 to 3 percent of the variance in utilization by Medicare beneficiaries (Schauffler, Howland, and Cobb, 1992). While up to 50 percent of the variation in outpatient expenditures is thought to be predictable, maximum R^2 levels for models of inpatient expenditures are extremely low (Newhouse et al., 1989). Much of inpatient utilization is truly random and unpredictable in the sense that hospitalizations are often due to unexpected, unusual acute health events of relatively short duration. Thus, the same individual may have widely varying inpatient use from year to year. Welch (1985) suggests that the 80 percent of health care expenditures which is unpredictable is due to transitory health events, while the 20 percent that is predictable represents a more permanent component of health, including chronic health conditions. Thus, the behavioral model, though its overall explanatory power is fairly low, has as much predictive power as any model of utilization developed to date.

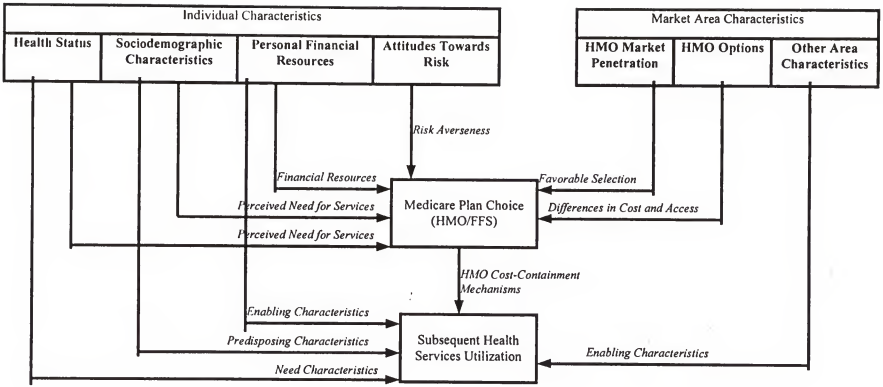
The utilization model in this study is based upon the behavioral model. Variables were selected to represent predisposing, enabling, and need characteristics of Medicare beneficiaries. Predisposing characteristics include age, sex, marital status, race, and level of educational attainment. Based on theory and the results of previous studies, one would expect higher utilization by individuals who are older, male, white, and highly educated (Gruenberg, Tompkins, and Porell, 1989; Garfinkel, Riley, and Iannacchione, 1988; Brown and Langwell, 1988). Enabling variables in this study include income, rural or urban nature of the county, county physician-to-population and hospital bed-to-population ratios, and whether the individual has a usual source of care. Previous studies have found these variables to be statistically significant predictors of

utilization (Garfinkel, Riley, and Iannacchione, 1988; Brown and Langwell, 1988). Need characteristics include nursing home residence, disability status, self-perceived health status, functional limitations, and chronic conditions. As noted earlier, these need characteristics consistently achieve statistical significance in utilization studies (Garfinkel, Riley, and Iannacchione, 1988; Epstein and Cumella, 1988; Thomas et al., 1984; Roos and Shapiro, 1981; Lubitz, Beebe, and Riley, 1985).

3.3. Conceptual model

The theoretical considerations discussed above lead to the following conceptual model of Medicare plan choice and subsequent health services utilization:

Figure 3.3. Conceptual Model



Following the health plan choice theories just presented, plan choice is modeled as a function of health status, sociodemographic characteristics, personal financial resources, attitudes towards risk, HMO market penetration, and available HMO options. Health status and sociodemographic characteristics shape an individual's perceived need for services and the importance she attaches to differences in cost and access between Medicare HMOs and fee-for-service plans. Personal financial resources also affect the weighting of cost and access considerations, as does the beneficiary's attitude toward risk. Unfortunately, although risk attitudes are part of the conceptual model, it is impossible to empirically model risk averseness with the datasets used in this study.

Plan choice is affected by Medicare HMO market penetration in that beneficiaries residing in high-penetration areas can reasonably be expected to be more familiar than individuals in areas with few HMO enrollees with the concept of an HMO, the availability of the Medicare HMO option, and the specific differences in cost and access to care that exist between HMOs and fee-for-service practice settings. The decision to enroll in an HMO is also influenced by the HMO options available to the beneficiary. For example, beneficiaries may be more likely to join an HMO if there is an open-panel HMO offering service than if only closed-panel HMOs are available, or if there is an HMO offering a prescription drug benefit or a zero-premium plan. These HMO characteristics affect the costs and benefits of HMO membership, and thus enter into beneficiaries' calculations of the differences in cost and access between health plans. It should be noted that although the above model does not include levels of deductibles and copayments in the HMOs, since theoretically these

variables do affect the utility calculations beneficiaries make in deciding whether to join and HMO or remain in a fee-for-service plan. Unfortunately, the available datasets do not include information on these variables.

In accordance with the behavioral model of health services utilization, health services use is conceived of as a function of need, predisposing, and enabling characteristics. The need characteristics are represented by health status measures. The predisposing characteristics are represented by sociodemographic variables. The enabling characteristics include personal financial resources and market area supply characteristics. Also, because previous empirical work suggests that HMOs are able to effect reductions in utilization relative to fee-for-service plans even after controlling for differences in the baseline health of their subscribers, utilization is modeled as a function of plan choice.

The empirical specification of this conceptual model is described in detail in Section 4.

4. Hypotheses and empirical model

4.1. Hypothesis 1: Favorable selection in Medicare HMOs

Hypothesis 1 posits that the amount of favorable selection in Medicare risk HMOs in a county will vary inversely with Medicare HMO market penetration, and that the overall strength of favorable selection observed in Medicare HMOs in this study will be quite weak, in contrast to the strong effects observed in the 1980s, when the total share of the Medicare market captured by HMOs was lower. As discussed earlier, there are four reasons for this relationship. First, since there are only a limited number of low-risk individuals in the Medicare population, HMOs in high-penetration markets must seek to recruit higher-risk individuals in order to keep increasing their market share. Second, Medicare beneficiaries in relatively poor health will be more disposed to join an HMO in markets with high HMO penetration. Consumers in these mature markets are more likely than consumers in low-penetration markets to be familiar with the concept of managed care, and they are more likely to have several HMOs to choose among. Third, higher rates of physician contracting with HMOs in high-penetration areas mean that chronically ill individuals will be less likely to have to leave their regular physician in order to join an HMO. Fourth, high-penetration markets typically have had Medicare HMOs operating in them for a long time, and the enrolled portion of the population in those markets will have aged and become sicker over time.

The following specific relationships between explanatory variables and the probability of HMO participation are expected to be statistically significant:

- Age (negative)
- Income (negative)
- Urban residence (positive)
- Disability-related Medicare eligibility before age 65 (negative)
- Self-perceived health status (positive)
- Ability to perform Activities of Daily Living (positive)
- Ability to perform Instrumental Activities of Daily Living (positive)
- Presence of major chronic conditions (negative)
- Health limitations on social life (negative)

Hypothesis 1 is tested by modeling health plan choice as a function of sociodemographic and health status characteristics and Medicare HMO market penetration. Probit regression is used because the dependent variable is dichotomous. A full discussion of the estimation method is given in Section 4.4.3. The model specification is as follows:

$$C_i = \alpha_0 + \alpha_1 H_i + \alpha_2 D_i + \alpha_3 F_i + \alpha_4 P_i + \alpha_5 O_i + \alpha_6 R_i + u_{1i} \qquad (1)$$

where

C = Health plan choice (1=continuously enrolled in a risk HMO; 0=continuously enrolled in fee-for-service)
H = Vector of health status variables
D = Vector of sociodemographic characteristics
F = Income
P = Vector of HMO market penetration variables
O = Vector of HMO options available in county
R = County AAPCC rate.

The vectors of health status, sociodemographic, market penetration, and HMO options variables are described in Table 4.1. The coefficients on the health status variables

indicate whether favorable selection persists in Medicare HMOs, and the coefficients on the interactions of these variables with HMO market penetration indicate whether the amount of favorable selection varies with the extent of market penetration.

4.2. Hypothesis 2: Utilization in Medicare HMOs

Hypothesis 2 states that Medicare HMO enrollees will have lower post-enrollment utilization than non-enrollees when sociodemographic and health status differences are controlled for. Studies of the early years of the Medicare risk program found that the HMOs achieved moderate reductions in inpatient and outpatient expenditures. However, since these studies did not control well for selection bias, it is likely that some of this observed effect was actually due to favorable selection. This study uses stronger statistical techniques to control for selection bias, and it is hypothesized that the results concerning the impact of HMOs on utilization consequently will be more modest than those of earlier studies as a result.

This hypothesis is tested using a system of simultaneous equations. The health plan choice equation described above is estimated and predicted values of the plan choice variable are then included in additional equations which model the utilization of HMO enrollees and non-enrollees. This technique accounts for the endogeneity that exists between health plan choice and utilization, and enables one to separate out effects on utilization due to the HMO practice setting from effects due to differences in the health of the HMO and fee-for-service populations (i.e., the biased selection). If there are unobservable factors which affect both HMO choice and subsequent utilization, estimation of the utilization equation using simple probit or ordinary least squares

would result in biased and inconsistent parameter estimates due the nonzero correlation between the HMO righthand-side variable and the error term. A full discussion of the simultaneous equations approach is given in Section 4.4.1. Identification is achieved through exclusion of the HMO market penetration variables from the utilization equations.

Both inpatient and physician services utilization are modeled as part of this system. Inpatient utilization is modeled using a two-part model. This consists of two functionally separable estimations of the probability of hospitalization and the number of inpatient days conditional upon hospitalization. The model specification is as follows:

$$\Pr(U_i>0) = \hat{\beta}_0 + \beta_1\hat{C}_i + \beta_2 H_i + \beta_3 D_i + \beta_4 F_i + \beta_5 R_i + \beta_6 A_i + u_{2i} \quad (2)$$

$$\ln U_i \mid U_i>0 = \theta_0 + \theta_1\hat{C}_i + \theta_2 H_i + \theta_3 D_i + \theta_4 F_i + \theta_5 R_i + \theta_6 A_i + u_{3i} \quad (3)$$

where

U = Number of hospital days
C = Predicted value of health plan choice variable, from Equation (1)
A = Vector of geographic area characteristics.

Use of physician services is modeled as follows:

$$V_i = \partial_0 + \partial_1\hat{C}_i + \partial_2 H_i + \partial_3 D_i + \partial_4 F_i + \partial_5 R_i + \partial_6 A_i + u_{4i} \quad (4)$$

where V is a binary indicator of whether or not the individual had at least one physician visit during the year.

In each of these utilization equations, the coefficient on the health plan choice variable (C) is the primary parameter of interest. It represents the independent effect of the HMO practice setting on utilization, after controlling for selection effects.

To validate the results of the simultaneous equations analysis, the above models are also estimated using a discrete factor model (Goldman, 1995). This semiparametric model allows exploration of endogenous relationships without rigid assumptions about the distribution of the errors. Additional information is provided in Section 4.4.2.

4.3. Operationalization of variables

4.3.1. Health plan choice equation

Variables for the health plan choice model were selected on the basis of the theory of health plan choice presented in Section 3. This theory posited that Medicare beneficiaries decide whether or not to participate in an HMO by assessing differences in cost and access between the HMO and the fee-for-service options, and weighting the importance of these differences based on three considerations: (1) their perceived likelihood of needing various types of health services; (2) their attitudes toward risk; and (3) their personal financial resources. Moreover, the likelihood that an individual will join an HMO is influenced by market area characteristics, including the available HMO options and HMO market penetration.

The operationalization of these variables is described in Table 4.1.

Table 4.1. Operationalization of the HMO Choice Conceptual Model

Theoretical Constructs and Variables	Type	Source	Definition
DEPENDENT VARIABLE:			
Health plan choice	Binary	MCBS	Full year in HMO or full year in fee-for-service
INDIVIDUAL CHARACTERISTICS:			
(1) Perceived need for services:			
• Age	Categ.	MCBS	3 categ.: 65-74, 75-84, and 85+
• Education	Categ.	MCBS	3 categ.: <high school, high school, college
• Race	Categ.	MCBS	3 categ.: white, black, other
• Sex	Binary	MCBS	Male/female

• Marital status	Binary	MCBS	Married or not married
• Nursing home residence	Binary	MCBS	Ever been a nursing home resident
• Disability *	Binary	MCBS	Disabled Medicare status
• Usual source of care	Binary	MCBS	Has a usual source of care
• Self-perceived health	Categ.	MCBS	*Data from year t-1, where t is the year of the health plan choice being modeled.* 3 categ.: excellent, good or fair, poor
• Activities of Daily Living (ADL)	Contin.	MCBS	*Data from year t-1.* Number of ADL person can perform out of 6 (bathing, dressing, walking, getting in/out of chairs, eating, toilet)
• Instrumental Activities of Daily Living (IADL)	Contin.	MCBS	*Data from year t-1.* Number of IADL person can perform out of 6 (shopping, paying bills, preparing meals, light housework, heavy housework, using phone)
• Chronic conditions*	Binary	MCBS	*Data from year t-1.* 16 binary variables indicating whether or not person has ever had each of 16 chronic conditions.
• Health limits social life	Binary	MCBS	*Data from year t-1.* Whether health limited the individual's social life in the past month
(2) Attitudes toward risk	--		Not represented
(3) Personal financial resources:			
• Income	Categ.	MCBS	4 categ.: <$10K, $10-30K, $30-50K, >$50K
• Medicaid status	Binary	MCBS	Medicaid recipient
• Medigap status	Binary	MCBS	*Data from year t-1.* Whether individual had a Medigap policy in the previous year.
MARKET AREA CHARACTERISTICS:			
(1) Available HMO options:			
• Availability of IPA option	Binary	PHP	IPA model Medicare HMO in county
• Availability of HMO w/drug benefit*	Binary	PHP	HMO w/prescription drug benefit in county
• Availability of a zero-premium plan*	Binary	PHP	Zero-premium Medicare HMO in county
(2) HMO market penetration:			
• Number of HMOs in county	Contin.	MMP	Number of risk HMOs offering service to Medicare beneficiaries in county
• HMO penetration	Contin.	MMP	% of county Medicare eligibles in risk HMOs
• Interaction terms	Contin.	MMP, MCBS	Interactions of HMO penetration with 6 health status and sociodemographic variables
(3) Control variables:			

* Rural/urban residence	Categ.	ARF	Rural-urban continuum code. Originally 9 categories, recoded to 7 categories.
* Census region	Categ.	ARF	Census region - 9 regions
* AAPCC rate	Contin.	MMP	Monthly AAPCC payment to HMOs in county, modeled as deviation from the mean
* Year	Categ.	MCBS	Year of data: 1993, 1994, 1995, or 1996

MCBS = Medicare Current Beneficiary Survey
PHP = Prepaid Health Plans Monthly Report File
MMP = Medicare Market Penetration File
ARF = Bureau of Health Professions Area Resource File
* = Estimation problems necessitated the respecification or exclusion of this variable. See Section 6.

The dependent variable, plan choice, is a binary variable indicating a full twelve months of enrollment in either an HMO or a fee-for-service plan. Individuals who switched between plans during the year are dropped for the year in which they switched (but retained for the years prior to and subsequent to the switch). It would not be appropriate to include these individuals in the switching year because they are not representative either of HMO enrollees or of fee-for-service stayers, and their utilization could obscure effects due to HMO practice setting.

On the righthand side of the choice equation are the variables described in the conceptual model of HMO choice. An individual's perceived need for services is a function of both her actual health status and her understanding of her health and the potential for medical care to improve it. Since both health status and the degree of understanding is likely to vary for individuals of different ages, levels of education, races, and so on, it is important to include sociodemographic characteristics in the model. Unfortunately, no variables are available in the datasets to represent

individuals' attitudes towards risk. This aspect of the conceptual model consequently had to be excluded from the model's empirical specification.

Personal financial resources are measured using an income variable and an indicator of whether the individual receives Medicaid benefits. Income is modeled as a series of dummy variables not because a linear relationship is implausible but because the Medicare Current Beneficiary Survey collects income data by categories rather than as a continuous variable. The income and Medicaid variables do not fully capture the concept of personal financial resources because they do not measure wealth. There are no variables in the dataset which describe savings, home ownership, or other aspects of wealth. This is a limitation of the empirical model.

The model also includes as a measure of financial resources an indicator of whether or not the individual had a Medigap policy in the previous year. Enrollment in a fee-for-service Medigap plan prior to making the decision whether or not to join an HMO could affect the HMO choice in three ways. First, if the individual's employer or former employer was paying for all or nearly all of the cost of the Medigap plan, the individual might be very reluctant to switch to an HMO (unless it was a zero-premium plan with more extensive benefits than the Medigap plan). Second, if the individual was paying high premiums out of pocket for a Medigap plan, he might be more interested in switching to an HMO than an individual who was not paying Medigap premiums. Third, there might be an inertia effect, such that persons who have been enrolled in a fee-for-service Medigap plan for a long time would be less likely to switch to an HMO than persons who do not have Medigap.

In addition to the above-described variables derived from the theory of health plan choice, the plan choice equation also includes variables measuring TEFRA HMO market penetration in the county. As described in Table 4.1., market penetration is modeled using a variable for the percentage of Medicare beneficiaries in a county who are HMO members and a variable for the number of TEFRA HMOs offering service in the county. The percent penetration variable is interacted with several of the health status variables to determine whether high managed care market penetration changes the relationship between health status and HMO choice – that is, whether market penetration has an impact on the degree of favorable selection into HMOs in a county.

A variable for the county AAPCC payment rate is also included in the HMO choice equation. The AAPCC rate has been found to be positively associated with HMO market penetration, because HMOs will choose to locate in areas where they will receive higher reimbursement from the government. In addition, HMOs operating in areas with relatively high AAPCC rates may be able to provide a more generous benefits package at a lower price to enrollees than HMOs receiving a lower level of reimbursement from HCFA. The AAPCC rate may thus affect the attractiveness of HMOs to Medicare beneficiaries and the likelihood that a beneficiary will choose to join. It is included in the model both as a control variable for this reason and as a proxy measure of market penetration.

4.3.2. Utilization equations

The variables for the inpatient and physician services utilization equations were selected based on the behavioral theory of health services utilization presented in

Section 3.2. This conceptual model postulates that health care consumption is determined by predisposing, enabling, and need characteristics. Table 4.2 describes how each of these types of characteristics are operationalized.

Table 4.2. Operationalization of the Utilization Conceptual Model

Theoretical Constructs and Variables	Type	Source	Definition
DEPENDENT VARIABLES:			
• Probability of hospitalization	Binary	MCBS	Hospitalized at least once during the year
• Conditional inpatient use	Contin.	MCBS	Total number of inpatient days for those hospitalized
• Physician utilization	Binary	MCBS	At least one physician visit during the year
PREDISPOSING CHARACTERISTICS:			
• Age	Categ.	MCBS	As in Table 4.1
• Education	Categ.	MCBS	As in Table 4.1.
• Sex	Binary	MCBS	As in Table 4.1.
• Race	Categ.	MCBS	As in Table 4.1.
• Marital status	Binary	MCBS	As in Table 4.1.
ENABLING CHARACTERISTICS:			
• Health plan choice	Binary	MCBS	As in Table 4.1.
• Medigap coverage	Binary	MCBS	Had a Medigap policy in the current year.
• Usual source of care	Binary	MCBS	As in Table 4.1.
• Income	Categ.	MCBS	As in Table 4.1.
• Medicaid status	Binary	MCBS	As in Table 4.1.
• Rural/urban residence	Categ.	ARF	As in Table 4.1.
• County physician: population ratio	Contin.	ARF	Ratio of number of physicians to population
• County hospital bed: population ratio	Contin.	ARF	Ratio of number of hospital beds to population
• County AAPCC rate	Contin.	MMP	As in Table 4.1.
• Census region	Categ.	ARF	As in Table 4.1
NEED CHARACTERISTICS:			
• Nursing home residence	Binary	MCBS	As in Table 4.1.
• Self-perceived health	Categ.	MCBS	As in Table 4.1.
• Activities of Daily Living (ADL)	Contin.	MCBS	As in Table 4.1.
• Instrumental Activities of Daily Living (IADL)	Contin.	MCBS	As in Table 4.1.
• Chronic conditions	Binary	MCBS	As in Table 4.1.
• Health limits social life	Binary	MCBS	As in Table 4.1.
CONTROL VARIABLES:			
• Year	Categ.	MCBS	As in Table 4.1.

MCBS = Medicare Current Beneficiary Survey; PHP = Prepaid Health Plans Monthly Report File;
MMP = Medicare Market Penetration File; ARF = Bureau of Health Professions Area Resource File

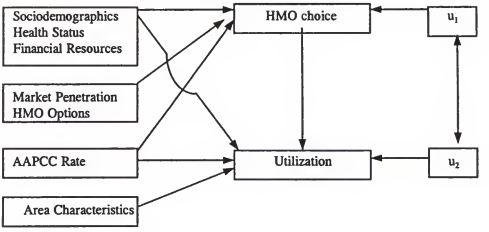
The continuous inpatient utilization variable is logged in the model in order to reduce the non-normality of its distribution. Age is modeled as two dummy variables to reflect the "inverse J-curve" effect as explained in Section 3.2.

4.4. Justification of analytical methods

4.4.1. Simultaneous equations

The use of simultaneous equations takes account of the endogeneity between the choice to participate in a Medicare HMO and subsequent utilization which exists due to biased selection into HMOs. There are likely to be omitted variables representing some unobserved aspect of health risk or propensity to use health care services which affect both the HMO choice and utilization. These omitted variables are represented by the error terms in the two equations. Because the same omitted variables will be part of the error terms in both equations, the error terms of the HMO choice and utilization equations will be correlated. The endogeneity problem is described by the following path diagram:

Figure 4.1
Path Diagram of Endogenous Relationships



Although estimation of the first equation (HMO choice) is straightforward because it has only predetermined variables on the righthand side, estimating the utilization equation using ordinary least squares regression (OLS) would result in biased and inconsistent estimates. The correlation in the errors of the two equations means that the HMO choice variable on the righthand side of the utilization equation is correlated with the error term of that equation, violating a key OLS assumption, $\text{cov}(X_i, u_i) = 0$.

In contrast, the simultaneous equations approach assumes nonzero covariance between the error terms of the equations [$E(u_{1i}u_{2i}) = \sigma_{12}$]. Additional assumptions of the model are as follows:

$E(u_{1i}) = E(u_{2i}) = 0$ [Mean of the errors in each equation is zero]

$E(X_i u_{1i}) = 0$ and $E(X_i u_{2i}) = 0$ [Zero covariance between regressors and errors]

$E(u_{1t}^2) = \sigma_{11}$ and $E(u_{2t}^2) = \sigma_{22}$ [Homoscedastic errors]

$E(u_{1i}u_{1s}) = 0$ and $E(u_{2i}u_{2s}) = 0$ [No autocorrelation].

To ensure that these assumptions are satisfied, several specification tests are performed. A formulation of the Hausman test (Hausman, 1976; Nakamura and Nakamura, 1981) described by Pindyck and Rubinfeld (1981) is used to examine the extent to which simultaneity occurs in the model. A Huber/White correction to the regression standard errors is used to correct for heteroscedasticity. Wald tests are also performed to confirm that irrelevant variables are not included in the model.

In order to estimate a system of simultaneous equations, the system must be identified. Identification refers to whether or not it is possible to obtain estimates of the structural equation parameters from the reduced form coefficients. Reduced form

coefficients are obtained from estimation of an equation with only predetermined variables on the righthand side. The issue of identification is essentially an issue of whether the number of reduced form parameters is less than, equal to, or greater than the number of structural parameters. When the number of reduced form parameters is less than the number of structural parameters, the equation is underidentified and cannot be estimated. When the numbers of parameters are equal, the equation is exactly identified and unique estimates of the structural parameters can be obtained from the reduced form parameters. When the number of reduced form parameters exceeds the number of structural parameters, the equation is overidentified, and more than one estimate of the structural parameters can be obtained from the reduced form parameters.

Identification can be achieved through several means, but in this study it is obtained through exclusion restrictions. Two predetermined market penetration variables (number of Medicare HMOs in the county and percent penetration), one HMO options variable (whether or not there is an IPA model Medicare HMO in the county), and six interaction terms (market penetration interacted with various health status measures) that are included in the HMO choice equation are excluded from the utilization equations. One can examine the order and rank conditions of identification to determine whether or not the utilization equation is identified through these exclusions. The order condition reveals the utilization equations each to be overidentified [(K-k)-(m-1) = (71-59)-(2-1) = 11]. The rank condition is satisfied with

a 1x1 matrix of nonzero determinants, confirming that the equations each are overidentified.

Originally it was intended that two additional HMO options variables, one representing whether or not there is a zero-premium HMO in the county and one representing whether there is an HMO offering a prescription drug benefit, would also be used as exclusion restrictions. However, when these variables were included in the HMO equation, a problem of perfect prediction arose. If the county in which the individual resided did not have a zero-premium plan available, in all cases the individual was not an HMO member. Similarly, if the county did not have an HMO offering a prescription drug benefit, in all cases the individual was not an HMO member. Because these two variables both perfectly predicted the outcome, they had to be excluded from the model.

Two specification tests are employed to test the validity of the exclusion restrictions used. First, a test described by Norton and Staiger (1994) examines whether the variables used to identify the model are correlated with the endogenous variable, i.e., whether they belong in the HMO equation. The HMO equation is run including the instruments, and a Wald test is used to test their joint significance. Second, another variation of the Wald test is used to test whether the variables used as overidentifying restrictions are correlated with the dependent variable in the second-stage equation, i.e., whether they really belong in the utilization equations. The utilization equation is run including the instruments, and if a Wald test reveals them to

be jointly significant, this is evidence that they are not valid overidentifying restrictions.

It can be argued that in addition to being identified through exclusion restrictions, the conditional inpatient days equation is identified by functional form, a method suggested by Dubin and McFadden (1984). The HMO choice variable is dichotomous and is estimated using a nonlinear approach. Predicted probabilities of HMO choice are then used as instruments to linearly estimate the inpatient days equation. Therefore, the inpatient days equation can be said to be identified by functional form. The identifying assumption of this strategy is that the predetermined variables affect HMO choice in a nonlinear way, but they affect inpatient utilization in a linear way (Norton and Staiger, 1994). However, this approach to identification has been widely criticized, and should be viewed as merely a supporting supplement to the exclusion restriction approach already described.

Since the utilization equations are overidentified, two-stage least squares (2SLS), will be used to estimate the model. In general, 2SLS involves regressing the endogenous variable (HMO choice, in this case) on only predetermined variables, and then including the predicted values of the endogenous variable in the second equation in lieu of the actual values of that variable. Since predicted rather than actual values of the endogenous variable are used in the second equation, this variable will not be correlated with the error term of the second equation and OLS may be used. 2SLS will produce consistent estimates in overidentified equations (but may have problems in small samples). 2SLS is often referred to as "instrumental variables estimation"

because the predicted values of the endogenous variable are used as "instruments" or proxies for the actual variable. The test described above by Norton and Staiger (1994) is used to examine whether the predicted probabilities of HMO participation constitute a good instrument.

Specification tests are performed to determine whether the predicted probabilities of HMO participation constitute a good instrument. A good instrument is one which is highly correlated with the endogenous variable (HMO choice) but uncorrelated with the error term of the second equation. Norton and Staiger (1994) discuss how the predicted probabilities from the HMO choice equation can be tested for correlations with actual data on HMO choice. If they are good instruments, they will be the only significant regressors in a least squares regression of actual HMO choice on predicted HMO choice and all other predetermined variables. Additionally, one can run the HMO choice model (using probit estimation) with the predicted probabilities on the righthand side and use a Wald test to test the significance of the predicted values.

Because both the endogenous variable and two of the utilization variables to be modeled in this study – the probability of hospitalization and the probability of a physician visit – are dichotomous, two-stage probit estimation will be employed in addition to 2SLS. As is discussed below, estimation of a dichotomous dependent variable using OLS is not appropriate and a nonlinear approach such as probit is preferred. Two-stage probit involves estimating the HMO choice equation using a probit model, and then using the predicted probabilities of HMO participation as instruments in the utilization equation.

Two-stage probit and standard 2SLS estimations in statistical packages such as STATA produce standard errors for the second stage regression that are not adjusted to account for the fact that the true endogenous variable of interest (HMO choice) is not used in the second stage. Rather, a proxy consisting of the predicted probabilities of HMO choice is used. The resulting standard errors are correct for the actual regression run, but not for the true model of interest. This is demonstrated by Gujarati (1995). Gujarati describes a simple correction factor by which the computer-generated standard errors in regular 2SLS should be adjusted in order to account for the use of the instrument. However, for two-stage probit estimations the formula for obtaining asymptotically correct standard errors is complex and requires one to have an estimate of the correlation of the errors for the two equations (Maddala, 1983; Bollen, Guilkey, and Mroz, 1995). Monte Carlo experiments conducted by Guilkey, Mroz, and Taylor (1992) did not reveal any gain in large finite samples from adjusting the standard errors. Bollen, Guilkey, and Mroz (1995) used the unadjusted standard errors in their two-stage probit estimation of contraceptive use in Tunisia. Following this work, the present study will use the unadjusted standard errors. While it would be desirable to correct the second-stage standard errors in the 2SLS system (i.e., in the conditional inpatient days equation), software limitations preclude this. STATA does not report the sum of squared errors as part of its panel data estimation commands (including XTREG and XTGEE). Consequently, it is not possible to estimate the correction factor for the standard errors. The standard errors should, however, be correct in the discrete factor model.

4.4.2. Discrete factor model

To provide confirmatory support for the results of the simultaneous equations analysis, health plan choice and utilization are also modeled using a discrete factor model (Goldman, 1995). This analysis is particularly important in the event that the simultaneous equations model fails one or more of the statistical tests for validity of the exclusion restrictions, exogeneity, validity of the instrumental variables, and satisfaction of the assumptions of the model. Even if the simultaneous equations model passes these tests, the discrete factor model serves to provide interesting insight into the robustness of the study results to changes in the assumptions about the distribution of the error term.

The discrete factor model is a semiparametric model which has been used by Goldman (1995) to model the endogenous relationship between health plan choice and health care utilization. Goldman describes a three-equation system very similar to the system proposed for the present study. The first equation models health plan choice (HMO/PPO or fee-for-service), the second models the probability of having some utilization, and the third models the level of utilization conditional upon use. The errors for each equation are decomposed into correlated and uncorrelated components. The discrete factor model assumes the uncorrelated components to be distributed normally. However, it assumes a discrete distribution for the correlated components. "A variable number of discrete incidental parameters can be used to approximate the distribution of the correlated components" (Goldman, 1995). The magnitude and sign of the correlations between the correlated components of the errors indicate the degree

and direction of biased selection into HMOs based on unobservable characteristics. For this study, a logit-based DFM program was used.

The advantage of the discrete factor model is that it relaxes the parametric assumptions of the traditional instrumental variables approach to modeling endogenous relationships. Goldman (1995) notes that these assumptions may be the reason that previous studies of the interdependence of health plan choice and utilization have sometimes failed to identify a selection effect in HMOs.

4.4.3. Probit model

Probit regression is used in this study to model the dichotomous dependent variables HMO choice, probability of hospitalization, and probability of having a physician visit. While ordinary least squares regression would be more straightforward and provide more interpretable coefficients, OLS is inappropriate for estimating probabilities for a number of reasons. First, the linear probability model (LPM) produces conditional expectations $[E(Y_i|X_i)]$ that lie outside the 0,1 range. These probabilities are meaningless. Second, the errors in the LPM will be heteroscedastic, violating a crucial OLS assumption. Since the dependent variable can only take on values of zero or one, the errors can only take on two values, $-X_i\beta$ (when the dependent variable is equal to zero) or $1-X_i\beta$ (when the dependent variable is one). Therefore, the variance of the errors, $E(u_i^2)$, is $-X_i\beta(1-X_i\beta)$, which is not constant. In the face of heteroscedasticity, OLS estimates will be unbiased but not minimum variance. Moreover, estimates of the variance of the parameter estimates will be biased and so confidence intervals and hypothesis tests will not be valid. Although weighted

least squares may be employed to adjust the standard errors, the LPM is still an undesirable model because it constrains the relationship between the dependent variable and the regressors to be linear. This may not be a reasonable assumption when the dependent variable is a probability.

For all of these reasons, a nonlinear approach is to be preferred to the LPM. This approach is motivated by the random utility model described in Section 3.1. To recap briefly, the random utility model involves an unobservable variable y_i^* which represents the difference in the utilities an individual derives from two possible alternatives (e.g. HMO participation or fee-for-service participation):

$$y_i^* = U_{i1} - U_{i2}$$

If the individual is observed to select alternative 1, then $U_{i1} > U_{i2}$, so $y_i^* > 0$. If he chooses alternative 2, then $U_{i1} < U_{i2}$, so $y_i^* < 0$. A regression model can be specified in which y_i^* is a function of a vector $\mathbf{X}$ of income, price, and taste variables:

$$y_i^* = \mathbf{X}_i \beta - u_i$$

However, since y_i^* is not observed, it is represented by a dummy variable y_i which takes on a value of one if $y_i^* > 0$ and a value of zero otherwise. The individual's choice is then modeled as:

$$\Pr(y_i = 1) = \Pr(y_i^* > 0) = \Pr(u_i < \mathbf{X}_i\beta) = F(\mathbf{X}_i\beta),$$

where F is the cumulative distribution function for u_i.

The two cumulative distribution functions most commonly used in binary choice models are the logistic (the logit model) and the normal (the probit model). These two distributions have very similar shapes, differing mainly in the thickness of the tails.

Predicted probabilities and significance levels of regressors are quite similar whether logit or probit is used, and in practice the choice between these two models usually boils down to a question of familiarity and ease of interpretation. The probit model is used in this study because it has been used more frequently than logit in two-stage estimations in the published literature.

The probit model estimates the probability that the dependent variable takes on a value of one as:

$$\Pr(Y_i=1) = \Phi(X_i\beta)$$

where Φ is the cumulative distribution function of the normal distribution. The likelihood function for the probit model is thus

$$L = \prod_{i=1}^{N}\left[\Phi(X_i\beta)\right]^{Y_i}\left[1-\Phi(X_i\beta)\right]^{-Y_i}.$$

The β coefficients in the probit model are estimated through maximum likelihood estimation (MLE), which involves maximization of the log likelihood function. MLE chooses from the array of all possible parameter values those values which are most likely to produce the observed data. Maximum likelihood estimates are consistent, asymptotically unbiased, asymptotically efficient, and asymptotically normally distributed. However, MLE may be biased in small samples.

These properties of MLE rest on assumptions of homoscedasticity and a correctly specified model. If the errors are heteroscedastic, maximum likelihood estimates will be inconsistent and the variance-covariance matrix will not be correct. If relevant variables are omitted from the model, causing specification error, MLE will produce inconsistent parameter estimates on the included variables. Omitted variable

bias may be detected through a Wald, likelihood ratio, or Lagrange multiplier test of restricted coefficients (Greene, 1993).

Parameter estimates in a probit model are not as directly interpretable as OLS estimates. The sign of a probit coefficient indicates the direction of the relationship between the regressor and the probability that the dependent variable equals one. The magnitude of the effect, however, can only be obtained through a comparison of probabilities at different values of the regressor of interest. Coefficients cannot be interpreted directly as the effect of a change in the regressor on the dependent variable. This is because the relationship modeled is nonlinear, and thus the magnitude of the effect of a change in X on $Pr(Y_i=1)$ depends on values taken on by all of the regressors. When $X_i\beta$ is large and negative or large and positive, $Pr(Y_i=1)$ increases slowly with $X_i\beta$. When $X_i\beta$ is near zero, $Pr(Y_i=1)$ increases quickly with $X_i\beta$. Therefore, it is important to demonstrate the effect of a change in X on Y by computing probabilities at specific values of X and comparing them.

4.4.4. Two-part model

Special techniques are needed to model inpatient utilization in the proposed study because of the distribution of this dependent variable. The distribution of hospital days is highly negatively skewed, with a large concentration of observations clustered at zero. Less than 20 percent of the individuals sampled in the Medicare Current Beneficiary Survey had a hospitalization in each of the years under study; over 80 percent had a zero value on this variable. Given this distribution, using OLS to model inpatient utilization would produce parameter estimates that were biased towards zero.

Two modeling techniques have been proposed to deal with the problem of data that are censored at zero: the tobit model (Tobin, 1958) and the two-part model (Duan et al., 1983, 1984). The tobit model has been criticized because it assumes that the errors are distributed truncated normal, with the truncation point at zero, which may not be a realistic assumption. Moreover, tobit assumes that the relationships between the regressors and the dependent variable are the same for persons with zero values of the dependent variable as for persons with uncensored values, which may also be untrue. Tobit also does not deal well with heteroscedasticity. For these reasons, the favored model for dealing with censored data is Duan and colleagues' two-part model, and that is the model employed in this study.

The two-part model consists of two functionally separable estimations. The first part of the model consists of a probit model in which the probability of having some utilization (e.g., at least one hospital day) is regressed on the righthand side variables:

$$\Pr(Y_i > 0) = f(\mathbf{X}_i) + u_i .$$

The entire sample is included in this regression. The second part of the model estimates the amount of utilization conditional on there having been some utilization. Only those persons who had at least one hospital day are included in this regression. The number of hospital days is regressed on the righthand side variables for this reduced sample using OLS:

$$E(Y_i \mid Y_i > 0) = f(\mathbf{X}_i) + v_i$$

Typically, the values of the utilization variable are logged in this conditional use equation to further reduce the non-normality in the distribution.

The two-part model produces two different sets of parameter estimates, one for the probit model and one for the OLS equation. Thus, the model permits discovery of different relationships between the regressors and inpatient utilization for persons who had no hospitalizations and for those who were hospitalized. Information from the two parts of the model can be combined to produce the expected value of the dependent variable for a particular individual: $E(Y_i) = Pr(Y_i > 0) * E(Y_i \mid Y_i > 0)$. The model also allows the composition of the vector of regressors to vary between the two parts of the model. An additional advantage is that the model is free from the assumption of a truncated normal error distribution made in the tobit model.

In this study, the two-part model will be integrated with simultaneous equations methods to produce a three-equation simultaneous system for the inpatient utilization variable:

$$C_i = \alpha_0 + \alpha_1 H_i + \alpha_2 D_i + \alpha_3 F_i + \alpha_4 P_i + \alpha_5 O_i + \alpha_6 R_i + u_{1i} \quad (1)$$

$$Pr(U_i > 0) = \beta_0 + \beta_1 \hat{C}_i + \beta_2 H_i + \beta_3 D_i + \beta_4 F_i + \beta_5 R_i + \beta_6 A_i + u_{2i} \quad (2)$$

$$\ln U_i \mid U_i > 0 = \theta_0 + \theta_1 \hat{C}_i + \theta_2 H_i + \theta_3 D_i + \theta_4 F_i + \theta_5 R_i + \theta_6 A_i + u_{3i} \quad (3)$$

where

- C = Health plan choice (1 = HMO enrollee)
- U = Inpatient utilization (hospital days)
- H = Vector of health status variables
- D = Vector of sociodemographic characteristics
- F = Vector of personal financial resources variables
- P = Vector of HMO market penetration variables
- O = Vector of available HMO options
- R = County AAPCC rate
- A = Vector of geographic area characteristics.

Equations (1) and (2) will be estimated using probit, and equation (3) will be estimated through OLS incorporating predicted values of the choice variable from equation (1).

The β_1 and θ_1 coefficients can be interpreted as the effect of the HMO practice setting on the probability of hospitalization and the number of inpatient days conditional upon hospitalization, respectively, controlling for any selection bias which might exist in the HMO choice.

4.4.5. Panel data analysis

The above models are run on a panel of four years of data (1993-96) from the Medicare Current Beneficiary Survey. There are two main advantages to a panel data approach. First, there are a limited number of HMO members in the MCBS sample. Including up to four years of data for each person significantly increases the effective size of the HMO sample. The inclusion of 1996 MCBS is particularly important because this round of the MCBS includes a special oversample of HMO enrollees.

The second advantage of the panel data approach is that it makes it possible to incorporate more information regarding individuals' health status and utilization than would be included in a cross-sectional analysis. Using only one year of data would raise the possibility that an individual's health services utilization and/or health status might be atypically high in that year due to some unusual health event. If the individual is essentially healthy and/or a low user of services (or if the individual has poor underlying health but atypically low use in one year), she can be expected to regress to the mean over time. Using four years of utilization and health status data would allow observation of this regression to the mean over a four-year period, and would provide a more accurate picture of the individual's general health and use patterns. Four years of data is likely to be sufficient to observe regression effects. Welch (1985) concluded

that for the sample he examined, there was a large regression effect within one year, and a lesser effect in subsequent years. He stated that "the more years of data, the better one's estimate of a group's permanent component of cost." This study incorporated four years of data for an unbalanced panel of 7,432 individuals (the "small panel" dataset). The analyses were also run on a larger unbalanced panel of 38,185 individuals (the "big panel" dataset) with between one and four years of data per person, as well as on a cross-sectional dataset.

The use of panel data requires that the models described above be altered slightly so that they take on a random effects specification:

$$C_{it} = \alpha_0 + \alpha H_{it} + \alpha_2 D_{it} + \alpha_3 F_{it} + \alpha_4 P_{it} + \alpha_5 O_{it} + \alpha_6 R_{it} + (u_{1i} + e_{1it}) \quad (1)$$

$$\Pr(U_{it} > 0) = \beta_0 + \beta_1 \hat{C}_{it} + \beta_2 H_{it} + \beta_3 D_{it} + \beta_4 F_{it} + \beta_5 R_{it} + \beta_6 A_{it} + (u_{2i} + e_{2it}) \quad (2)$$

$$\ln U_{it} \mid U_{it} > 0 = \theta_0 + \theta_1 \hat{C}_{it} + \theta_2 H_{it} + \theta_3 D_{it} + \theta_4 F_{it} + \theta_5 R_{it} + \theta_6 A_{it} + (u_{3i} + e_{3it}) \quad (3)$$

$$\Pr(V_{it}) = \partial_0 + \partial_1 \hat{C}_{it} + \partial_2 H_{it} + \partial_3 D_{it} + \partial_4 F_{it} + \partial_5 R_{it} + \partial_6 A_{it} + (u_{4i} + e_{4it}) \quad (4)$$

where

- i = Person index
- t = Year index
- C = Health plan choice (1=HMO enrollee)
- U = Inpatient utilization (hospital days)
- V = Physician visit
- H = Vector of health status variables
- D = Vector of sociodemographic characteristics
- F = Vector of personal financial resources variables
- P = Vector of HMO market penetration variables
- O = Vector of available HMO options
- R = County AAPCC rate
- A = Vector of geographic area characteristics

In the above specification, one observation is taken per person per year. Although it would be possible to take one observation per month, there would be no significant advantage to doing so because there is very little switching between HMO and fee-for-service plans on a month-to-month basis in this MCBS sample. Moreover, health service utilization measurements taken on a monthly basis would be even more likely to reflect high variation due to random events than utilization aggregated to an annual measurement.

The random effects model estimates a single intercept and constant slope coefficients for all individuals in the sample, but decomposes the error term into two components. An individual-specific component, the u_i, is constant over time and represents variations in utilization which can be attributable to permanent differences in the risk profile of persons in the sample. An random component, the e_{it}, varies over individuals and over time and represents the residual unexplained variation which exists due to the occurrence of unusual acute health events. The model rests on the following assumptions about the error term (McCall and Wai, 1983):

$E(u_i) = 0$	$E(e_{it}) = 0$	[Mean of the errors is zero]
$E(u_i^2) = \sigma_u^2$	$E(e_{it}^2) = \sigma_e^2$	[Homoscedasticity]
$E(u_i u_k) = 0$, i k	$E(e_{it}e_{ks}) = 0$, i k or t s	[No autocorrelation]

$E(u_i e_{kt}) = 0$ for all i, k, and t [Error components independent]

Let $v_{it} = u_i + e_{it}$. Then:

$E(v_{it}) = 0$	[Mean of the errors is zero]
$E(v_{it}^2) = \sigma^2 = \sigma_u^2 + \sigma_e^2$	[Homoscedasticity]

Because the variance-covariance matrix is not $\sigma^2 I$, the random effects model is estimated using the method of generalized least squares (GLS).

The random effects model also assumes that there are no omitted variables. If there are omitted variables which are correlated with the included regressors, random effects coefficients will be biased. This is the primary weakness of the model. It is a moderate concern in the present study because it is difficult to capture all dimensions of health risk in a survey such as the MCBS, and there may be aspects of risk which are unobserved but are correlated with the health status variables included in the model. However, there is reason to be confident that health risk is fairly well-represented in the model, as there will be measures of self-perceived health status, functional limitations, comorbidity, and specific diagnoses.

The random effects specification is preferred to the alternative, fixed effects, for this study for four reasons. First, random effects is preferred when the number of individuals in the sample is large (N-K 10) and the number of time periods is small ($T<3$). This is the case in the present study ($N-K>10{,}000$ and $T=4$). With large N and small T, and assuming no omitted variables, fixed effects parameter estimates are consistent but not asymptotically efficient. Second, random effects permits generalization beyond the particular sample studied, whereas fixed effects is conditional upon the sample.

Third, random effects is preferred to fixed effects when the estimation method for the model is probit. Greene (1993) notes that recent research in econometrics has demonstrated that a random effects specification of the probit model is feasible. This model is now available as part of the STATA 5.0 statistical package. In contrast, a fixed effects probit model cannot be estimated because "there is no feasible way to

remove the heterogeneity, and with large numbers of cross-sectional units, estimation of the [errors] is intractable." In the random effects probit specification, if the random component of the errors (the e_{it}) are taken to be independent standard normal variables, the panel nature of the data is irrelevant, and the standard methods involved in probit estimation may be employed.

4.4.6. Complex survey design adjustments

Because data from the Medicare Current Beneficiary Survey are used in this study, it is necessary to make some adjustments for the MCBS's complex survey design. The MCBS uses two-stage cluster sampling, which leads to a loss of precision (a larger sampling variance) relative to simple random sampling. This is because the clusters tend to be internally homogeneous, and observations within them correlated. Thus, the independent sample size is reduced, with attendant costs for the precision of regression estimates. A test procedure based on the assumption of simple random sampling will overstate the statistical significance of parameter estimates. In addition, because in a complex survey the random error terms are no longer independent of one another, the coefficients from regression based on the assumption of simple random sampling will be biased. The use of sampling weights removes the bias in the coefficients, but does not entirely remedy the bias in the variances of the coefficients. In order to conduct accurate tests of statistical significance, one must also adjust the standard errors to account for the design effect.

Sampling weights reflect the differential probabilities of each individual being selected into the sample; that is, the number of persons in the population which each

sample person represents. Cross-sectional weights indicate the probability of being selected into the sample in the year in which the person was selected. Longitudinal weights reflect not only the probability of initially being selected, but also the probability of having stayed in the sample over a multiyear period given that the person was selected. Longitudinal weights thus capture not only information about the number of persons in the population the sampled individual represents, but also the likelihood of the sampled person dying, being lost to follow-up, or being rotated out of the MCBS sample (the rotating panel design of the MCBS is explained more fully in Section 5).

Both cross-sectional and longitudinal weights are provided as part of the MCBS dataset. However, three-year backward longitudinal weights are only provided in the 1996 file for those individuals who have been in the sample for all four years (1993-96). Individuals who died, were lost to follow-up, or became newly eligible during that period of time do not have a weight assigned to them. Consequently, it is not possible to do a longitudinally weighted analysis on the full sample including deaths and newly eligibles. This is unfortunate since inclusion of persons who died is important in models estimating costs to the Medicare program (because health services utilization is very high in the last year of life).

To deal with this dilemma, it was decided to perform the analysis on three different datasets derived from the MCBS, with three different weighting schemes. Dataset 1, which will hereinafter be referred to as the "small panel" dataset, consists of those individuals for whom longitudinal weights are available in the 1996 file, i.e.,

those individuals who remained in the sample over the entire 1993-96 period. The longitudinal weights are used in the estimation.

Dataset 2, which will be called the "big panel" dataset, consists of every person included in *any* year of the MCBS from 1993 to 1996, including newly eligibles, persons who died, and persons who were lost to follow-up. Persons who died were included for the year of the death and all preceding years. Persons newly eligible for Medicare were included for all years for which they had a full year's data (i.e., not for the year in which they became eligible). This is because data collection is incomplete for the year of joining, and some data are imputed for these "ghosts."

The estimation for the big panel dataset is unweighted, because neither the longitudinal nor the cross-sectional weights provided in the MCBS files are appropriate for use with this sample. When sampling weights are not incorporated, regression coefficients may differ from the true population parameters and standard errors may be incorrect. However, in theory no sampling weights are needed in a multivariate model if the model is fully specified (i.e., has no specification error or omitted variables bias).

Dataset 3, which will be called the "cross-sectional" dataset, consists of the entire sample from the 1996 Access to Care file. A description of the Access to Care file is given in Section 5, but the salient feature for the purposes of this discussion is that the file contains only continuously enrolled persons, not deaths or newly eligibles. Thus, the cross-sectional dataset consists only of those persons who were continuously enrolled in 1996. Cross-sectional sampling weights are used in this estimation.

In addition to incorporating sampling weights, it is also necessary to make adjustments for the design effect of a complex survey in order to perform regression analysis and obtain correct standard errors. The design effect captures the difference in the precision of parameter estimates calculated from complex survey data compared to the precision of estimates calculated from data from a simple random sample. It is the ratio of the variance of the estimator based on the complex survey design to the variance of the estimator based on a simple random sample (SRS) of the same size:

$$DE = \frac{\text{variance of estimator in complex design}}{\text{variance of estimator in SRS}}$$

The design effect is usually greater than one, indicating that the complex survey design results in a loss of precision relative to SRS.

Because adjusting for the design effect involves considerable computational effort, a preliminary analysis of the significance levels using the unadjusted standard errors was undertaken in order to determine whether the design effect adjustment was really necessary in this study. It was thought that if few of the variables were near the borderline of statistical significance (p-values between 0.02 and 0.05), the design effect adjustment would not significantly impact the results. Details of this analysis are reported in Section 6, but in brief, the analysis revealed that no significant gains would be associated with the adjustment. Consequently, standard errors unadjusted for the design effect were used.

5. Data

5.1. Overview of dataset linkages

The analysis in this study was performed on linked data from four different sources: the Medicare Current Beneficiary Survey (MCBS), the Area Resource File, HCFA's Medicare Market Penetration File, and HCFA's Medicare Prepaid Health Plans Monthly Report. Details on the content of each of these datasets are presented in Sections 5.2 through 5.5.

Beneficiary-level information on sociodemographic and health status characteristics, HMO participation, and health services utilization was obtained from the MCBS. The MCBS Cost and Use Files for 1992-95 and the MCBS Access to Care File for 1996 were used. The Cost and Use File for 1996 was not yet available at the time the analysis was conducted.

County-level data on AAPCC rates and the number and market penetration of existing TEFRA HMOs were obtained from the Health Care Financing Administration's Medicare Market Penetration File. These data were supplemented with information on HMO characteristics (HMO model type, benefits, and premiums) from HCFA's Medicare Prepaid Health Plans Monthly Report. Both of these files are publicly available on the Internet.

The HMO data were supplemented with data from the Bureau of Health Professions Area Resource File on other county characteristics, including rural/urban status, supply of physicians and hospital beds. The Area Resource File is publicly

available on the UNIX mainframe at the University of North Carolina at Chapel Hill through the Inter-university Consortium on Political and Social Research. Finally, the county-level data was merged with the Medicare Current Beneficiary Survey data on beneficiary's county of residence.

5.2. The Medicare Current Beneficiary Survey

5.2.1. Sampling scheme

The Medicare Current Beneficiary Survey (MCBS) is a continuous, multipurpose survey by the Health Care Financing Administration of a representative sample of the Medicare population. Face-to-face, in-home interviews with Medicare beneficiaries are conducted three times a year beginning in 1991 to collect information on utilization of health services, medical care expenditures, health insurance coverage, sources of payment, health status and functioning, demographic information, and health beliefs and practices (United States Department of Health and Human Services, Health Care Financing Administration, 1996a). Interview data are supplemented with linked Medicare claims data and selected administrative data.

The original MCBS sample of 15,411 aged and disabled Medicare beneficiaries was selected in 1991 to be representative of the Medicare eligible population on January 1, 1991. Beneficiaries were selected from the HISKEW (Health Insurance SKEleton Write-off) file based on a standard 5 percent sample of HCFA's Enrollment Data Base. Excluded from the sample population were Medicare enrollees who were residents of foreign countries and U.S. possessions other than Puerto Rica, persons

who first enrolled in Medicare after January 1, 1991, and persons who disenrolled or died prior to the end of December 1991.

A three-stage cluster sampling scheme was employed. The sample was drawn from 107 primary sampling units (PSUs), which are metropolitan areas or clusters of non-metropolitan counties chosen to represent the United States. The second stage involved selection of initially 1,163 ZIP code clusters (expanded to 1,399 in the 1993 MCBS round). In the third stage, Medicare beneficiaries were selected within the sample ZIP code clusters (United States Department of Health and Human Services, Health Care Financing Administration, 1996a).

Because some respondents were lost to follow-up after Round 1 due to death, emigration, and nonresponse, the original 1991 sample was supplemented with additional respondents at each round in order to maintain the desired sample size. A supplement of 2,410 beneficiaries were added in Round 4 (1992). In the 1993 round, an additional 2,449 newly-enrolled and previously-enrolled beneficiaries were added, bringing the Round 7 sample size to 12,863 persons. Subsequent rounds have added additional persons to the sample.

5.2.2. Data reconciliation process and underreporting issue

MCBS data are presented in two different types of files: Access to Care and Cost and Use. Access to Care is publicly available, while permission from HCFA must be obtained to use Cost and Use because it contains confidential identifiers which have been deleted from Access to Care. The major difference between the two files is that Cost and Use includes utilization information for persons who enrolled in

Medicare during the year or died during the year, while Access to Care includes only those individuals who were continuously enrolled in Medicare for the entire year. Both Access to Care and Cost and Use contain survey data supplemented with Medicare claims data and other data from HCFA's administrative files. However, unlike the Access to Care file, the Cost and Use file has undergone a careful reconciliation process to match interview, claims, and administrative data (United States Department of Health and Human Services, Health Care Financing Administration, 1996b). Health care services that are reported from both interview and claims data are reported separately from those identified from bills or interview responses alone. Cost and Use contains information on all health care services received whether covered by Medicare or not; in contrast, Access to Care does not include services such as prescription drugs and long term facility care which are not covered by Medicare. Thus, Cost and Use provides a more complete and accurate picture of utilization and expenditures than Access to Care.

The matching process employed to integrate interview, billing, and administrative data in the Cost and Use file is complex and thorough. A complete description of the matching methodology is given in Eppig and Edwards (1996). The purpose of record matching is to adjust for survey underreporting and to fill in and correct survey reported payment amounts with more accurate information from bills submitted to and paid by Medicare (United States Department of Health and Human Services, Health Care Financing Administration, 1996b). Matching, of course, can only be done for those services which are covered by Medicare, because HCFA does not have bills for non-covered services. Additionally, it is not possible to perform

matching for beneficiaries enrolled in HMOs, since HMOs do not submit claims. This problem, and the bias it may introduce into measurements of utilization, will be discussed shortly.

Over 192,000 health care events from fee-for-service Medicare bills were examined in the matching process. About 104,000 (54 percent) of these were successfully matched to survey records. About 76,000 events reported in the surveys could not be matched to billing records under the "strength of evidence" criteria and hierarchical algorithms used. The 76,000 unmatched events would be expected to include some part of the unmatched bill events which could not be matched under the criteria used, as well as a significant number of events that are not covered by Medicare.

In addition to correcting for health care events that were not reported in the survey, the matching process also helped to fill in missing Medicare payment amounts and to correct Medicare payment amounts that had been reported incorrectly. The reconciliations generated corrections that made Medicare a payer on the 22 percent of cases where this information was omitted from the survey report and made it possible to determine the correct Medicare payment amount in 39 percent of the survey records in which this information was missing (United States Department of Health and Human Services, Health Care Financing Administration, 1996b). Where Medicare bills could not be used to correct payment amounts, a complex imputation process was used to fill in estimated payments. This involved imputing a reasonable total for the service based on similar cases in the file, checking which payers were possible based on reported insurance information, and utilizing a computerized iterative imputation

technique which borrowed from both Gibbs sampling and and "hot deck" imputation methods to supply missing payment data for likely payment up to the target reimbursement amount.

Because billing records were not available for enrollees in Medicare HMOs, there is a strong possibility of bias in the utilization variables. Fee-for-service beneficiaries' reported utilization was validated through the matching process, but survey reports of utilization by HMO enrollees were not. Any reporting error which might exist in the survey was not corrected for these enrollees. HCFA examined the possibility of under- or over-reporting of utilization in the survey and concluded that, based on the corrections made for fee-for-service beneficiaries, substantial underreporting had occurred (United States Department of Health and Human Services, Health Care Financing Administration, 1996b). The fact that there were 88,000 unmatched health care events in the billing records indicates that respondents failed to recall a significant proportion of their utilization.

The underreporting problem appeared to be more serious for event counts than for Medicare payments. Moreover, minor health care events appeared to be less likely to be reported than major events. The 88,000 Medicare events in the billing records constituted 46 percent of all events but only 25 percent of total Medicare expenditures, suggesting that events forgotten in the survey tended to be less expensive than those remembered. There also is some evidence that survey respondents overstated the amount paid for their care. HCFA examined 63,000 health care events that had both a survey and a bill record, and found that the Medicare payment reported in the survey overstated the amount recorded in the billing records by $5.7 billion, and the total

payment reported in the survey overstated the total payment noted in the Medicare bill records by $16.4 billion (United States Department of Health and Human Services, Health Care Financing Administration, 1996b). This overreporting is one of the reasons that payments are not used as a utilization variable in the present study.

The underreporting of health care events, however, remains a problem for this study. Other researchers have confirmed that interview respondents are likely to understate their health services utilization. Loftus and colleagues (1992) collected survey and medical records data on 211 HMO enrollees and found that respondents underreported the number of physician visits they had had in the previous twelve months by 13 percent. Jobe and colleagues (1990) determined that interview respondents underreported the number of visits to medical providers by 20 percent. Cannell and Fowler (1963) conducted face-to-face interviews in which individuals were asked to recall their hospital use over the past year. Comparing interview responses to hospital records, they found that only 83 percent of hospitalizations were reported in the interviews, and length of stay was reported accurately for only 42 percent of hospitalizations. A literature review by Marquis (1984) confirmed that other studies have also found evidence of underreporting of hospital use. More recently, a literature review by Stearns and colleagues (1996) highlighted Reuben and colleagues' (1991) finding that only 70 percent of the claims in Medicare administrative records were reported during the National Health and Nutrition Examination Surveys (NHANES) I Epidemiologic Followup Study, and the very similar results reported by Calore and Lim (1989) for the National Medical Expenditure Survey (NMES). Stearns and colleagues' (1996) study of Longitudinal Survey of Aging (LSOA) data found the

number of hospitalizations reported in the interviews actually exceeded those recorded in claims data. However, this appears to have been due to an administrative glitch rather than true overreporting. The frequency and magnitude of reporting errors does not appear to vary significantly with the age of the respondent (Sherbourne and Meredith, 1992; Rodgers and Herzog, 1987; Herzog and Dielman, 1985).

Studies show that errors in recall during interviews become more likely as the length of the recall period increases (Wentland and Smith, 1993). A study of 1,500 people who had been discharged from the hospital during the previous year found that nearly all respondents remembered the hospitalization if asked about it within 2 1/2 months of the discharge date, but the percentage dropped over time, and less than 90 percent remembered it if asked a year later (Public Health Service, 1965). Respondents in the Medicare Current Beneficiary are asked to recall only the last 4 months, rather than the 10 or 12 months used in other studies cited herein. A recent study of the accuracy of survey responses by AIDS patients using a 4-month recall period (Weissman et al., 1996) found that survey responses did provide valid data, with mean overall reporting errors small and not significantly different from zero, but that high utilizers tended to underreport all types of services. The validity of MCBS survey responses is augmented by the use of insurance records and health care calendars in the interviews. The use of such prompts is known to increase the accuracy of responses (Cannell and Fowler, 1963; Wentland and Smith, 1993).

Despite these mitigating factors, there remains the possibility of significant underreporting of both inpatient and outpatient utilization by MCBS respondents. The inability of HCFA to adjust HMO enrollees' responses to correct for underreporting

means that the measurements of their utilization are likely to be biased towards zero, while those of fee-for-service beneficiaries will be substantially less biased. As a consequence, the magnitude of the difference in utilization between HMO enrollees and fee-for-service beneficiaries may be overstated.

Two steps were taken to assess and minimize the impact of this weakness in the data. First, in selecting the specific outpatient utilization variable to use as the dependent variable in Equation 4, care was taken to select the variable least likely to have significant underreporting bias. The variable selected is a simple binary indicator of whether or not the individual consulted a physician at least once during the year. Survey respondents are much more likely to be able to recall whether or not they saw a doctor at all than to be able to remember the exact number of visits, the amount paid for physician services, or the number of Part B claims filed. Moreover, because the data for this variable come from unreconciled survey reports, rather than claims data or reconciled survey-claims data, underreporting bias should be the same magnitude for HMO and fee-for-service enrollees. A HCFA statistician knowledgeable about the survey process and dataset structure of the MCBS was consulted to confirm that this variable was the best choice for minimizing underreporting bias. An unfortunate result of the decision to use this variable is that deaths and newly eligibles had to be excluded from the outpatient model for the big panel dataset. This is because those individuals are not in the Access to Care File, the file which contains the binary physician variable used for the analysis. Consequently, they are missing data on that variable.

The second measure taken to assess the magnitude of this bias is an analysis of the extent of underreporting the inpatient utilization variables. The inpatient variables used in the analysis come from Medicare claims data, rather than survey reports. While it was not thought appropriate to use claims data for the outpatient variable because HMOs rarely submit "no-pay bills" for physician visits, it is less problematic to use claims data on inpatient use because there is generally a reasonable level of HMO reporting of hospitalizations. To determine the magnitude of underreporting, unreconciled claims data on the number of hospitalizations was compared to reconciled claims-survey data on the number of hospitalizations for a panel of individuals from the 1993-95 Cost and Use Files. It was not possible to perform this comparison for number of inpatient days because there is no reconciled inpatient days variable. The results of the comparison for number of hospitalizations were that 5356 of the 5456 individuals (98.24%) had exactly the same value for both the claims-only and the reconciled variables; 79 (1.45 percent) had a one-event difference; and 21 (0.0039 percent) had a difference of two or more events. Nearly all of the event differences were in the direction of claims data underreporting the "true" (reconciled) value. The very high match rate indicates that claims data reliably reports inpatient utilization for number of hospitalizations. Based on these results, it was thought to be reasonable to conclude that the claims data are probably also highly reliable for number of inpatient days. Thus, underreporting does not appear to be a big problem for the inpatient utilization variable in this study.

5.2.3. Other validity issues

Other than the underreporting problem, MCBS data are thought to have a high degree of validity. The strengths of the MCBS's sampling and data collection designs and the analytic strengths of the resulting data have been documented in the literature (Adler, 1994). The MCBS was developed by HCFA's Office of the Actuary based on an extensive body of experience in government health surveys, including the Current Medicare Survey and the National Medical Expenditure Survey. The MCBS is unique in covering the entire continuously enrolled Medicare population, oversampling certain subpopulations, and following and reinterviewing the sample to obtain a continuous longitudinal picture (Adler, 1994). Response rates have been high, rising from 87.2 percent in Round 1 to 97.3 percent in Round 7.

A number of steps are taken to ensure data quality in the MCBS. Interview data concerning diagnoses, procedures, and covered charges are validated using linked Medicare claims files where possible. Data for non-respondents to the interview are also available in the files for purposes of comparison with respondents on salient characteristics and calculations of sample weights. Accurate recall of health care information is encouraged through the use of respondents' Medicare and insurance statements, supporting bills, receipts, and prescriptions, as well as a calendar respondents are asked to keep to record details of care. The interviewer is assisted by the CAPI (Computer-Assisted Personal Interviewing) system, which was found in pilot tests to greatly increase the efficiency of the questionnaire and reduce interviewer errors (Adler, 1994). The CAPI also edits responses as they are collected, ensuring

that answers to questions are consistent with one another (e.g. a person described as a "son" must be male). Errors remaining after the CAPI review are examined closely by the nine full-time staff members.

MCBS measures of health and functional status have been tested for face validity. The MCBS assesses general health status by asking respondents, "In general, compared to other people your age, would you say that your health is excellent, very good, good, fair or poor?" Responses to this question, although it asks for a subjective judgment, have been found to be predictive of objective health status, health behaviors, and mortality (Idler, 1992; Adler, 1995). The validity of MCBS measures of physical functioning, activities of daily living and instrumental activities of daily living, is also well-documented (Wiener and Hanley, 1989). Research has also demonstrated that ADLs are reliably and objectively measurable from survey data (Wiener and Hanley, 1989).

5.3. The Medicare Market Penetration File

The Medicare Market Penetration File is comprised of administrative data from the Health Care Financing Administration on enrollment in TEFRA HMOs. Data from the December 1993, 1994, 1995, and 1996 files were used. Data are not publicly available for years prior to 1993.

The dataset contains a relatively small number of variables: county name, state name, HMO plan number and name, HCFA contract type (risk, cost, etc.), number of Medicare entitled beneficiaries residing in the county, number of Medicare managed care enrollees in the county, percent penetration (TEFRA HMO enrollees as a fraction

of the Medicare entitled population), and the total Part A and B AAPCC rate. Plan names were used to construct a variable indicating the number of risk HMOs serving the county. Plan names and plan numbers were also used to link data with the Medicare Prepaid Health Plans Monthly Report File, while county name was used to link data with the ARF and MCBS. Details on the merging procedures are given in Section 5.6.

Unfortunately, very little documentation (1/2 page) is provided with Medicare Market Penetration File, so it is difficult to evaluate the strengths and weaknesses of the dataset. The data should be highly accurate since the variables are relatively simple and come from HCFA's own records.

5.4. The Medicare Prepaid Health Plans Monthly Report File

The Medicare Prepaid Health Plans Monthly Report File contains administrative data from HCFA concerning the characteristics of HMOs with active contracts with HCFA. Among the information provided is a list of risk contractors by region, a summary of the benefits and premiums offered by each risk contractor, and a description of each HMO's model type (IPA, group model, or staff model). This file was used to construct variables describing the HMO options available to a Medicare beneficiary residing in a given county who is considering whether or not to join an HMO. In particular, information about the model type, benefits, and premiums offered by each plan was linked with the plan names/numbers in the Medicare Market Penetration File to create county-level variables for "IPA available in the county",

"HMO with drug benefit available in the county", and "zero-premium HMO plan available in the county."

The scant documentation provided with the Prepaid Heath Plans Monthly Report File does not permit an assessment of the dataset's strengths and weaknesses.

5.5. The Area Resource File

The 1998 Bureau of Health Professions Area Resource File is a county-based dataset which summarizes secondary data from a variety of sources. The file contains over 7,000 variables for all U.S. counties except those in Alaska (United States Department of Health and Human Services, Health Resources and Services Administration, 1995). Variables include county descriptor codes, health professions and health professions training data, health facility data, county-level utilization and health care expenditure data, population data (including sociodemographic and income data), and environmental data.

Only four variables are used in this study: county identifier, rural/urban continuum codes, county physician-to-population ratio, and county hospital bed-to-population ratio. The rural/urban continuum codes are multi-category classification system distinguishing metropolitan counties by size and nonmetropolitan counties by degree of urbanization and proximity to metropolitan areas. The codes are a finer measure of urbanization than the Metropolitan Statistical Area (MSA) code, and are particularly suited to studies of HMO enrollment and health services utilization because they capture effects due to the adjacency of a rural county to a more metropolitan county. There is no *a priori* reason to doubt the validity of the rural/urban continuum

codes. The categories are based on meaningful population, urbanization, and location criteria. The data from these codes were drawn in 1994.

Information on physician-to-population and hospital bed-to-population ratios should also have an acceptable degree of validity. The population data used are 1990 census data. Data on physicians practicing in the counties were obtained from the 1993 American Medical Association Physician Masterfile. This file is more useful than census data because it distinguishes between practicing MDs and others, including interns and residents, chiropractors, dentists, and veterinarians, who may list "doctor" as their profession on census surveys. Data on the supply of hospital beds in the county are derived from the 1992 American Hospital Association Annual Survey of Hospitals. The reliability of this data is only as good as the hospitals' responses to the survey (United States Department of Health and Human Services, Health Resources and Services Administration, 1995). In 1992, 659 of 6,664 hospitals surveyed (9.9 percent) failed to respond. For non-respondents, the AHA reported previously-gathered data for many of the variables. Thus, the validity of data from this survey can be considered good, but not impeccable.

5.6. Key decisions made in the dataset merging process

The creation of the dataset used in the study was a complex process involving several important discretionary decisions. The dataset consists of data extracted from the MCBS and merged with a special county-level HMO dataset. The procedure used to create the merged dataset is outlined below.

5.6.1. The MCBS dataset

The MCBS side of the dataset was created by merging MCBS data from the Access to Care Files for 1993-1996 and the Cost and Use Files for 1993-95. The MCBS data were obtained in tape form from HCFA pursuant to a valid Data Release Agreement. They were converted to SAS dataset form by a programmer at the Cecil G. Sheps Center for Health Services Research. Selected variables were extracted from each of the files, and these single-year datasets were merged on individual identifier to create a dataset with multiple observations per individual. Individuals were excluded from the sample based on the following criteria: age under 65; residence outside the continental United States; End Stage Renal Disease status; and "ghost" status. "Ghosts" in the MCBS are individuals who were not interviewed over the full four rounds, and for whom some data are thus imputed.

After the MCBS dataset was merged with the county-level HMO dataset, as described below, three additional exclusion criteria were applied. First, if the individual resided in a county that was not served by at least one Medicare risk HMO in that year, the individual was dropped from the sample for that year. This was necessary because it is nonsensical to model the choice of whether or not to join a Medicare HMO for persons who did not have the option of joining. Second, individuals in Medicare HMOs other than risk HMOs (e.g., cost HMOs) were excluded from the sample because the structure of economic incentives facing cost HMOs is not the same as that facing risk contractors. Third, individuals who switched into or out of a risk HMO during the year were excluded from the analysis for that year only. Their inclusion for that year would result in crossover bias, i.e., if HMOs

dramatically reduce utilization but an individual spent only four months of the year in an HMO, his/her level of utilization might be much higher than expected and would not reflect the true cost-saving effects of HMOs.

5.6.2. The county-level HMO dataset

The county-level HMO dataset was created by merging data from HCFA's Medicare Prepaid Health Plans Monthly Reports, HCFA's Medicare Managed Care Market Penetration quarterly data files, and the Area Resource File. The following procedure was used:

1. The Medicare Prepaid Health Plans Monthly Reports for December 1993, December 1994, December 1995, and December 1996 were downloaded from HCFA's website.

2. For all risk plans, data on plan name, contract number, contract type, drug benefits, premiums, and model type were re-entered by hand in STATA. Each entry was triple-checked against a hard copy of the original dataset. The following validations and/or adjustments were made to the raw data:

- If a contractor offered more than one kind of plan under the same contract number, the plans were treated as a single contract. A "yes" value was entered for the variable "plan offers a drug benefit" if *any* of the plans under a single contract number offered a drug benefit.
- The original datasets listed only one premium amount per contract number, even if the contractor offered multiple plans. According to a representative from HCFA, there are no data available to determine whether contractors actually offer the

different plans at different premiums. However, the HCFA representative agreed that it was reasonable to assume that if a contractor did not offer a zero-premium basic plan, it was unlikely that any of their additional "flexible option" plans, which have more extensive benefits, were zero-premium plans either. Thus, a contractor was coded as not offering a zero-premium plan if its basic plan had a nonzero premium.

- Cost contractors and Health Care Prepayment Plans (HCPPs) were excluded from the dataset, but risk demonstrations, risk plans with cost enrollees remaining, and HCPPS converted to risk plans were included. There are no premium data available for demonstration plans, which creates a weakness in the dataset. It is a potential problem if the only plan in the county that was an IPA model or a zero-premium plan or offered a drug benefit was a demonstration plan. Since that plan would be missing data, the dataset would show a zero value for each of these variables for that county, when in reality it should be a value of one. An analysis of the data for 1996 revealed that this could be a potential problem in 20 counties for that year.
- There were several risk plans in each year that were missing data on benefits offered. These data were imputed where they were available for that plan for the year preceding or following the missing year. Where data from prior or subsequent years were not available, they were coded as a missing value. This is a weakness of the dataset, since it means that the dummy variables "zero-premium plan available in this county," "drug benefit offered in this county" and "IPA operating in this county" may be set to zero when in reality there was a plan with these

characteristics operating there but its data were missing. Each plan with missing data was investigated individually, but HCFA was unable to explain why data were missing for these plans, and could not provide any alternative means of obtaining this data. The counts for the numbers of plans with non-imputable missing data are as follows: 18 in 1993, 18 in 1994, 12 in 1995, and 12 in 1996.

3. The Medicare Managed Care Market Penetration quarterly data files for December 1993, December 1994, December 1995, and December 1996 were downloaded from HCFA's website. A number of the counties in each year were missing data on AAPCC rate, the number of Medicare eligibles, and/or the number of HMOs operating in the county. HCFA could not provide an explanation as to why these data were missing. Several of these counties were not necessary to the analysis because they turned out not to contain any MCBS respondents. However, even after the exclusion of those counties, there were 55 counties missing data. These counties had to be dropped, as it was not possible to get the data from an alternative source.

4. The market penetration dataset was supplemented with SSA state/county codes from the HCFA state and county crosswalk file for the Prospective Payment System.

5. The two plan-level datasets were merged on contract number.

6. Plan-level data within each county were aggregated to create a county-level dataset.

7. The FIPS county crosswalk file was used to recode in the 1998 Area Resource File the FIPS codes of extinct counties and cities that are now part of counties

(several cities in Virginia). Alaskan counties were dropped since they were missing both ARF and market penetration data, and no MCBS respondents live in Alaska.

8. The ARF data was merged into the county-level HMO dataset on FIPS code.

9. The completed county-level dataset was merged with the completed MCBS dataset on FIPS code. This dataset is the "big panel" dataset.

10. The "small panel" dataset was created by dropping individuals in the big panel dataset who were not in the sample all four years. The cross-sectional dataset was created by dropping all data from 1993-95 from the big panel dataset.

5.6.3. Data cleaning

As was discussed in Section 5.2.3, MCBS data already have undergone multiple edits by the time they reach the user. In addition to automatic edits by the CAPI system, the data are carefully cleaned by MCBS personnel. For the purposes of this study, the data were checked again thoroughly for coding errors. The mean, minimum, and maximum of each variable was examined to ascertain their reasonableness, and the counts for each value of the variable were checked against the counts recorded in the MCBS codebook.

After ensuring that the data were clean, several recodes were performed in order to customize the variables for the models in this study. The HMO choice variable was created by summing the monthly managed care plan enrollment indicators in the MCBS. The binary choice variable was coded 1 if the individual was a risk HMO member for all 12 months of the year, and 0 if the individual had no HMO enrollment during the year. As explained above, individuals who had 1 to 11 months

HMO enrollment in a given year were dropped from the sample for that year. Persons who died during the year (in 1993-95) were coded as being HMO members (HMO choice = 1) if they were enrolled in an HMO for every month prior to their death.

The inpatient days variable was rescaled to account for the fact that persons who died during the year did not have a chance to incur a full year's utilization. The following formula was used to rescale inpatient days: newdays = (olddays/number of months of data) * 12. This formula simply extends over the full year the individual's recorded level of utilization for the months for which data was gathered.

The nursing home residence variable had to be specially constructed because nursing home residence is indicated differently in the Cost and Use File and the Access to Care File. For the 1992-95 sample, the Cost & Use File contains a facility residence indicator. For the 1996 (Access to Care only) sample, however, it was necessary to use an "interview type" indicator as a proxy for facility residence. The Access to Care file indicates whether the interview was conducted in a nursing home or in the community. This is not a perfect measure of institutional status, though, because it only codes as nursing home residents those individuals who were in a nursing home at the time of the interview. However, since the interviews take place every four months, it is unlikely that there are very many people who are actually nursing home residents for most of the year but happened to be interviewed during a period in which they were out. A similar problem was encountered with the Medigap variable, as Access to Care and Cost and Use report this information differently. However, in this case both of the Medigap variables are good measures.

Lagged values of the health status variables (self-perceived health, ADLs, IADLs, health limitations on social life, and chronic conditions) and the Medigap variable were used in the HMO choice model. However, some individuals in the sample did not have data on these variables available for the previous year, because they were not in the sample the previous year. For these individuals, data from the current year were used.

Descriptive statistics for all variables were examined to check the number of missing observations. For variables missing data on fewer than 100 observations in a given year, observations that were missing data were dropped. For variables missing data on more than 100 observations, an analysis was performed to determine whether missingness was a random event. A dummy variable indicating missingness was regressed on all the righthand side variables in the HMO and utilization models to determine whether any of the variables was significantly associated with missingness.

Data were missing for several hundred observations on the variables Medigap status, education, and income. The above analysis determined that missingness was nonrandom (i.e., was associated with several of the righthand side variables) for each of these variables. Consequently, dummy variables for missingness were included in the models for these variables, along with the main effects, which were recoded such that missing values were changed to zeroes. The coefficient on the main effect in such a regression indicates the effect of the variable itself on the dependent variable, while the coefficient on the dummy missingness indicator controls for having replaced the missing values with zeroes. Aside from these three variables, there were no other variables for which there was missing data on more than 100 observations.

6. Results: Favorable selection and market penetration

6.1. Descriptive statistics

Descriptive statistics for the three datasets are presented in Table 6.1. A review of the descriptive statistics for the big panel dataset reveals a sample of fairly healthy, functional elderly Medicare beneficiaries of mean age 72.5 years. The observations in the dataset are approximately 60 percent female, 88 percent white, 49 percent married and 71 percent educated at the high school level or less. About 70 percent of the dataset observations resided large or medium sized metropolitan area. About 30 percentof the observations had an annual income below $10,000, and about 81 percent had an income below $30,000.

Initially, the dataset contained 972 "switchers" (2.45 percent) who had partial-year membership in both a risk HMO and a fee-for-service plan. These switchers were dropped for the year of their switching. In the final dataset, approximately 12 percent of the sample were Medicare risk HMO enrollees for the entire year, while 88 percent were in fee-for-service settings for the entire year. Sixty-four percent of the observations in the dataset had a Medigap plan, and approximately 13 percent were Medicaid recipients.

The individuals in the dataset generally perceived themselves to be in good health; only 7.5 percent reported that their heath status was poor. Except for hypertension and arthritis, chronic conditions were fairly rare. Most of the individuals reported being able to perform most or all of the Activities of Daily Living (ADLs) and

the Instrumental Activities of Daily Living (IADLs). Less than 20 percent of the observations in the dataset had one or more hospital days, and among those with at least one hospitalization, the mean number of inpatient days was 2.6 days. Approximately 78 percent of the dataset had at least one physician visit during the year.

Seven hundred twenty-five counties were represented in the dataset. Observationss were spread fairly evenly over the continental U.S., with some concentration in the Atlantic states. The mean county physician-to-population ratio among observations in the dataset was 2.6 physicians per thousand population; the mean hospital-bed-to-population ratio was 4.4 beds per thousand. Ninety-five percent of the observations were persons who lived in counties in which an IPA-model Medicare HMO was operating. The average number of Medicare HMOs operating in a county was 25, and the average market share of Medicare HMOs in a county was approximately 9 percent. Average market penetration rose from 5.0 percent in 1993 to 6.0 percent in 1994 to 8.0 percent in 1995 to 13.8 percent in 1996.

The variables "zero-premium plan in the county" and "plan offering drug benefit in the county" were dropped from the dataset before the final datasets were created, for the reasons discussed in Section 6.2. However, descriptive statistics from an earlier version of the big panel dataset give a rough indication of the percentages of individuals in the sample who lives in counties served by plans with these characteristics. Approximately 97 percent of the dataset observations were persons residing in counties served by a zero-premium plan, and 98 percent lived in counties served by a plan offering a prescription drug benefit.

Table 6.1. Sample Descriptive Statistics

Variable	Big Panel (38,185 observations, 21,965 persons)		Small Panel (7,432 observations, 2,243 persons)		Cross-sectional (13,373 persons)	
Year of observation						
• 1993	7,810	(20.45%)	1,071	(14.41%)	0	
• 1994	8,528	(22.33%)	1,997	(26.87%)	0	
• 1995	8,474	(22.19%)	2,157	(29.02%)	0	
• 1996	13,373	(35.02%)	2,207	(29.70%)	13,373	(100.0%)
HMO member	4,642	(12.16%)	696	(9.37%)	2,958	(22.12%)
Hospitalized at least once	7,338	(19.22%)	1,169	(15.73%)	2,171	(16.23%)
Number of inpatient days (for those hospitalized)	Mean 13.656 s.d. 21.256 Min/Max 1/365 Skewness 6.190 Kurtosis 63.401		Mean 10.320 s.d. 12.167 Min/Max 1/106 Skewness 3.577 Kurtosis 21.005		Mean 10.403 s.d. 12.907 Min/Max 1/150 Skewness 3.838 Kurtosis 25.337	
At least one physician visit	29,963	(78.47%)	6,091	(81.96%)	11,045	(82.59%)
Died during the year	911	(2.39%)	0		0	
Age						
• 65-74	15,388	(40.30%)	3,271	(44.01%)	5,755	(43.03%)
• 75-84	15,236	(39.90%)	2,848	(38.32%)	5,296	(39.60%)
• 85+	7,561	(19.80%)	1,313	(17.67%)	2,322	(17.36%)
Male	15,179	(39.75%)	2,933	(39.47%)	5,416	(40.50%)
Race						
• White	33,698	(88.25%)	6,458	(86.90%)	11,795	(88.20%)
• Black	3,466	(9.08%)	742	(9.98%)	1,198	(8.90%)
• Other nonwhite	1,021	(2.67%)	232	(3.12%)	380	(2.84%)
Education*						
• Less than high school	9,730	(25.48%)	2,042	(27.48%)	3,050	(22.81%)
• High school graduate	17,397	(45.56%)	3,369	(45.33%)	6,565	(49.09%)
• Some college	9,964	(26.09%)	1,883	(25.34%)	3,371	(25.21%)
• Missing	1,094	(2.87%)	138	(1.86%)	387	(2.89%)
Married	18,786	(49.20%)	3,756	(50.54%)	6,619	(49.50%)
Annual income*						
• Under $10,000	11,716	(30.68%)	2,204	(29.66%)	3,313	(24.77%)
• $10,000-$30,000	19,241	(50.39%)	3,909	(52.60%)	6,834	(51.10%)
• $30,000-$50,000	4,536	(11.88%)	891	(11.99%)	1,703	(12.74%)
• Over $50,000	1,861	(4.87%)	372	(5.01%)	692	(5.18%)
• Missing	831	(2.18%)	56	(0.01%)	831	(6.21%)
Medigap policy holder*						
• Yes	24,765	(64.86%)	5,130	(69.03%)	8,424	(62.99%)
• No	12,839	(33.62%)	2,224	(29.93%)	4,949	(37.01%)
• Missing	581	(1.52%)	78	1.05%)	0	
Medicaid recipient	4,881	(12.78%)	950	(12.78%)	1,516	(11.34%)
Nursing home resident	3,496	(9.16%)	496	(6.67%)	873	(6.53%)
Has a usual source of care	33,518	(8.78%)	6,538	(87.97%)	11,756	(87.91%)

Self-perceived health						
• Excellent or very good	16,157	(42.31%)	3,445	(46.35%)	5,917	(44.25%)
• Good or fair	19,157	(50.17%)	3,591	(48.32%)	6,535	(48.87%)
• Poor	2,871	(7.52%)	396	(5.33%)	921	(6.89%)
ADLs	Mean	4.95	Mean	5.35	Mean	5.09
	s.d.	1.77	s.d.	1.37	s.d.	1.65
	Min/Max	0/6	Min/Max	0/6	Min/Max	0/6
	Skewness	-1.66	Skewness	-2.36	Skewness	-1.90
	Kurtosis	4.51	Kurtosis	7.97	Kurtosis	5.47
IADLs	Mean	4.66	Mean	4.92	Mean	4.85
	s.d.	1.98	s.d.	1.78	s.d.	1.85
	Min/Max	0/6	Min/Max	0/6	Min/Max	0/6
	Skewness	-1.36	Skewness	-1.68	Skewness	-1.58
	Kurtosis	3.41	Kurtosis	4.58	Kurtosis	4.18
Health limits social life	13,816	(36.18%)	2,417	(32.52%)	4,338	(32.54%)
History of arthritis	21,729	(56.91%)	4,483	(60.32%)	7,185	(53.73%)
History of rheumatoid arthritis	5,166	(13.53%)	1,073	(14.44%)	1,505	(11.25%)
History of atherosclerosis	6,120	(16.03%)	1,187	(15.97%)	1,768	(13.22%)
History of broken hip	2,618	(6.86%)	451	(6.07%)	799	(5.98%)
History of cancer (other than skin)	7,540	(19.75%)	1,463	(19.69%)	2,386	(17.84%)
History of coronary heart disease	6,623	(17.35%)	1,257	(16.91%)	1,564	(11.70%)
History of skin cancer	6,846	(17.93%)	1,377	(18.53%)	2,394	(17.90%)
History of diabetes	6,150	(16.11%)	1,150	(15.47%)	2,004	(14.99%)
History of emphysema/asthma /COPD	5,425	(14.21%)	1,058	(14.24%)	1,789	(13.38%)
History of high blood pressure	20,590	(53.92%)	4,094	(55.09%)	6,970	(52.12%)
History of myocardial infarction	6,047	(15.84%)	1,095	(14.73%)	1,942	(14.52%)
History of osteoporosis	4,457	(11.67%)	944	(12.70%)	1,479	(11.06%)
History of heart problem other than MI and CHD	12,020	(31.48%)	2,233	(30.05%)	3,793	(28.36%)
History of Parkinson's Disease	784	(2.05%)	114	(1.53%)	241	(1.80%)
History of partial paralysis	2,585	(6.77%)	459	(6.18%)	692	(5.18%)
History of stroke	5,229	(13.69%)	909	(12.23%)	1,705	(12.75%)

Census division						
* New England	1,294	(3.39%)	251	(3.38%)	404	(3.02%)
* Mid Atlantic	6,966	(18.24%)	1,343	(18.07%)	2,258	(16.89%)
* East North Central	6,007	(15.73%)	1,227	(16.51%)	2,033	(15.20%)
* West North Central	2,479	(6.49%)	410	(5.52%)	727	(5.44%)
* South Atlantic	8,351	(21.87%)	1,631	(21.95%)	3,161	(23.64%)
* East South Central	1,889	(4.95%)	380	(5.11%)	576	(4.31%)
* West South Central	3,654	(9.57%)	738	(9.93%)	1,324	(9.90%)
* Mountain	2,584	(6.77%)	531	(7.15%)	995	(7.44%)
* Pacific	4,961	(12.99%)	921	(12.39%)	1,895	(14,17%)
Rural/urban residence						
* Large metro central	16,426	(43.01%)	3,196	(43.00%)	6,018	(45.00%)
* Large metro fringe	1,337	(3.50%)	277	(3.73%)	446	(3.34%)
* Medium metro	9,266	(24.27%)	1,804	(24.27%)	3,222	(24.09%)
* Lesser metro	2,959	(7.75%)	603	(8.11%)	855	(6.39%)
* Nonmetro urbanized	2,865	(7.50%)	560	(7.54%)	914	(6.83%)
* Nonmetro less urbanized	5,065	(13.26%)	948	(12.76%)	1,825	(13.65%)
* Thinly populated	267	(0.0070%)	44	(0.0059%)	93	(0.0070%)
Physician: population ratio	Mean	0.0026	Mean	0.00262	Mean	0.00258
	s.d.	0.0016	s.d.	0.00171	s.d.	0.0020
	Min/Max	0/0.0201	Min/Max		Min/Max	
	Skewness	1.59		0.00012/0.0201		0.00012/0.0201
	Kurtosis	8.11	Skewness	1.75	Skewness	1.67
			Kurtosis	9.73	Kurtosis	8.75
Hospital bed: population ratio	Mean	0.0044	Mean	0.0043	Mean	0.0042
	s.d.	0.0026	s.d.	0.0026	s.d.	0.0025
	Min/Max	0/0.0389	Min/Max	0/0.0244	Min/Max	0/0.0202
	Skewness	1.76	Skewness	1.74	Skewness	1.72
	Kurtosis	10.28	Kurtosis	8.91	Kurtosis	8.75
Monthly AAPCC payment (deviation from the mean)	Mean	3.41	Mean	1.34	Mean	48.18
	s.d.	101.68	s.d.	96.76	s.d.	104.87
	Min/Max		Min/Max		Min/Max	
		-215.75/337.66		-215.75/318.54		-199.99/337.66
	Skewness	0.61	Skewness	0.57	Skewness	0.45
	Kurtosis	3.28	Kurtosis	3.27	Kurtosis	2.98
IPA model HMO in county	36,164	(94.71%)	7,114	(95.72%)	13,103	(97.95%)
Number of HMOs in county	Mean	25.37	Mean	23.85	Mean	36.34
	s.d.	25.61	s.d.	23.16	s.d.	32.17
	Min/Max	1/110	Min/Max	1/110	Min/Max	1/110
	Skewness	1.44	Skewness	1.43	Skewness	0.87
	Kurtosis	4.57	Kurtosis	4.83	Kurtosis	2.61
HMO market penetration (%)	Mean	0.090	Mean	0.081	Mean	0.138
	s.d.	0.128	s.d.	0.119	s.d.	
	Min/Max		Min/Max		0.147	
		0.0000564 /0.516		0.0000673/0.516	Min/Max	
	Skewness	1.49	Skewness	1.63		0.000103/0.516
	Kurtosis	4.07	Kurtosis	4.68	Skewness	0.87
					Kurtosis	2.44

* = Missing observations are included in the descriptive statistics for this variable because missing observations were not dropped, but rather recoded, as previously described.

6.2 Model diagnostics

A number of estimation problems were encountered in the course of programming the models. A discussion of these issues and the means used to resolve them is provided for interested readers in Appendix A.

Several diagnostic tests were conducted to verify the appropriateness of the final model specification. Pairwise correlations were examined for all three datasets to detect possible collinearity problems in the model. None of the correlations were cause for serious concern. The highest observed correlations in all of the datasets were between IADLs and ADLs (in the 0.81-0.82 range); lagged IADLs and lagged ADLs (in the 0.80-0.81 range); and number of HMOs and percent market penetration (in the 0.78-0.81 range). Positive correlation coefficients between 0.50 and 0.64 were observed in all three datasets for AAPCC rate and number of HMOs, and for physician-to-population ratio and bed-to-population ratio. Negative correlations between -0.54 and -0.63 were observed in all three datasets for nursing home residence and IADLs; nursing home residence and ADLs; nursing home residence and lagged IADLs; and nursing home residence and lagged ADLs. In the small panel dataset only, there was a 0.51 correlation for the variables Medicaid recipient and income under $10,000, and a -0.52 correlation for Medicaid recipient and Medigap policyholder variables. In the cross-sectional dataset only, there was a 0.50 correlation between AAPCC rate and percent HMO market penetration. None of these correlations are surprising, and none are high enough to create a serious multicollinearity problem. While the correlations between ADLs and IADLs are high, it is well-established in the

literature that these two variables capture different dimensions of functional status and it is appropriate to include both in a model as controls for health status.

In order to correct for heteroscedasticity, Huber/White corrections were performed to the standard errors in all models. All of the diagnostic tests were performed on estimations including these corrected standard errors.

As has already been discussed, the specification of the HMO model was tested using a Wald test. Specifically, theoretical uncertainty about whether the 16 chronic conditions variables belonged in the HMO equation was investigated. The results of the Wald test for the HMO equation have already been described. The final HMO choice models retained only three of the chronic conditions variables: history of arthritis, history of cancer, and history of stroke.

The other diagnostics performed on the HMO equation are discussed in Section 7, as they relate primarily to the validity of inferences about the effects of HMO membership on utilization.

6.3. Big panel dataset results

Table 6.2 summarizes the regression results for the HMO choice equation for the big panel instrumental variables and discrete factor estimations, as well as for the small panel and cross-sectional estimations. Predicted probabilities are presented in Table 6.3. Because making predictions from discrete factor model coefficients is computationally onerous, only selected probabilities involving statistically significant variables are presented for that model.

Table 6.2. Probability of HMO Membership

	Big Panel IV (xtprobit) n=38,185 χ^2_{53}=6090.15 (p=0.0000)		Big Panel DFM (logit) n=38,185		Small Panel IV (xtprobit) n=7,432 χ^2_{53}=645.74 (p=0.0000)		Cross-Sectional (probit) n=13,373 χ^2_{50}=2294.04 (p=0.0000) Pseudo-R^2=0.4192	
	Coeff.	Robust s.e.	Coeff.	Robust s.e.	Coeff.	Robust s.e.	Coeff.	Robust s.e.
Constant	-1.422**	(0.201)	4.843**	(1.063)	-2.028**	(0.508)	-1.79**	(0.32)
Market penetration:								
Percent penetration	4.01**	(0.49)	21.069**	(2.104)	4.58**	(1.37)	4.05**	(0.70)
Number of HMOs	-0.00121	(0.00082)	-0.0345**	(0.0042)	-0.0005	(0.0029)	-0.0015	(0.0011)
Penetration*Excellent health[L]	-0.25	(0.17)	-0.86	(0.77)	-0.56	(0.51)	-0.24	(0.28)
Penetration*Poor health[L]	-0.001	(0.374)	0.49	(1.60)	-2.44	(1.41)	0.59	(0.54)
Penetration*Number of chronic conditions[L]	-0.115**	(0.036)	-0.33**	(0.13)	-0.12	(0.13)	-0.108*	(0.048)
Penetration*Health limitations on social life[L]	0.04	(0.19)	0.32	(0.88)	0.68	(0.45)	-0.07	(0.31)
Penetration*ADLs[L]	0.103	(0.089)	0.57	(0.38)	0.12	(0.26)	0.18	(0.14)
Penetration*IADLs[L]	-0.005	(0.088)	-0.03	(0.35)	-0.04	(0.24)	-0.20	(0.14)
IPA plan available	0.23	(0.15)	0.23	(0.73)	0.23	(0.15)	0.34	(0.23)
AAPCC rate	0.00109**	(0.00024)	0.0111**	(0.0014)	0.10	(0.36)	0.00081*	(0.00033)
Health and functional status:								
Excellent health[L]	0.116**	(0.039)	0.44*	(0.20)	0.139	(0.103)	0.110**	(0.071)
Poor health[L]	-0.092	(0.089)	-0.53	(0.42)	-0.63*	(0.26)	-0.26	(0.15)
ADLs[L]	0.0070	(0.0201)	-0.012	(0.095)	-0.072	(0.052)	-0.019	(0.038)
IADLs[L]	0.012	(0.020)	0.021	(0.091)	0.115*	(0.046)	0.047	(0.038)
Health limitations on social life[L]	-0.0654	(0.045)	-0.24	(0.23)	0.06	(0.12)	-0.024	(0.083)
History of arthritis[L]	0.074*	(0.029)	0.28*	(0.12)	0.058	(0.093)	0.104*	(0.043)
History of cancer[L]	-0.088*	(0.038)	-0.29	(0.16)	-0.12	(0.13)	-0.099	(0.058)
History of stroke[L]	-0.11158*	(0.05008)	-0.538**	(0.202)	-0.21*	(0.17)	-0.040	(0.066)
Demographics:								
AAPCC adjustors:								
Age 75 to 84	-0.094**	(0.023)	-0.47**	(0.12)	-0.101	(0.099)	-0.066	(0.043)
Age 85+	-0.161**	(0.044)	-0.87**	(0.18)	-0.16	(0.13)	-0.130*	(0.060)
Male	0.015	(0.030)	0.10	(0.12)	-0.04	(0.11)	-0.003	(0.043)

Medicaid recipient	-1.210**	(0.068)	-5.02**	(0.30)	-1.26**	(0.22)	-1.253**	(0.091)
Nursing home resident	-0.247**	(0.093)	-1.37**	(0.37)	-0.37	(0.24)	-0.13	(0.18)
Other:								
Less than high school education	-0.162**	(0.038)	-0.43**	(0.16)	-0.21	(0.14)	-0.147**	(0.057)
Some college	-0.115**	(0.034)	-0.41**	(0.14)	-0.18	(0.12)	-0.088	(0.049)
Medigap policy^{L}	-1.388**	(0.033)	-5.66**	(0.24)	-0.980**	(0.104)	-1.505**	(0.046)
Black	0.0027	(0.0508)	0.08	(0.20)	0.17	(0.17)	-0.207**	(0.075)
Other nonwhite race	-0.033	(0.086)	-0.37	(0.28)	-0.03	(0.25)	-0.23*	(0.11)
Married	0.0816**	(0.0308)	0.36**	(0.13)	-0.080	(0.110)	0.028	(0.046)
Has a usual source of care	0.491**	(0.059)	1.95**	(0.23)	0.74**	(0.16)	0.596**	(0.097)
Income <$10,000	-0.002	(0.035)	0.14	(0.14)	-0.002	(0.107)	-0.057	(0.059)
Income $30,001-$50,000	-0.1269**	(0.0404)	-0.52**	(0.16)	-0.046	(0.110)	-0.111	(0.065)
Income >$50,000	-0.285**	(0.065)	-0.89**	(0.25)	-0.112	(0.190)	-0.144	(0.096)
New England division	0.0745	(0.089)	0.53	(0.54)	0.15	(0.29)	0.18	(0.13)
Mid Atlantic division	-0.036	(0.049)	-0.405	(0.305)	-0.02	(0.15)	-0.113	(0.074)
East North Central division	0.008	(0.058)	-0.66*	(0.32)	-0.34	(0.18)	0.009	(0.085)
West North Central division	0.135	(0.075)	0.52	(0.64)	-0.32	(0.29)	0.04	(0.11)
East South Central division	-0.244*	(0.103)	-1.92**	(0.48)	-0.35	(0.29)	0.04	(0.14)
West South Central division	0.158**	(0.057)	0.02	(0.32)	0.23	(0.19)	0.220*	(0.084)
Mountain division	0.266**	(0.062)	2.46**	(0.32)	0.10	(0.25)	0.152	(0.090)
Pacific division	0.234**	(0.054)	1.25**	(0.26)	0.18	(0.20)	0.240**	(0.078)
Large fringe metro county	-0.198*	(0.086)	-0.64	(0.43)	-0.37	(0.33)	-0.15	(0.12)
Medium metro county	-0.040	(0.041)	-0.82**	(0.23)	-0.01	(0.16)	-0.0106	(0.0601)
Lesser metro county	-0.367**	(0.084)	-2.61**	(0.50)	-0.917**	(0.33)	-0.28*	(0.13)
Urbanized nonmetro county	-1.24**	(0.17)	-4.94**	(0.96)	-0.68*	(0.32)	-1.29**	(0.25)
Less urbanized nonmetro county	-0.597**	(0.074)	-3.682**	(0.41)	-0.37	(0.23)	-0.7370**	(0.1008)
Thinly populated county	-0.20	(0.17)	-2.62**	(0.97)	-0.85	(0.63)	-0.15	(0.22)
Control variables:								
Missing education data	0.24	(0.13)	1.32**	(0.39)	0.61	(0.33)	0.04	(0.15)
Missing Medigap data	-0.776**	(0.045)	-3.53**	(0.24)	-0.69**	(0.13)	-0.48	(0.37)
Missing income data	-0.114	(0.096)	-0.36	(0.33)	-0.25	(0.51)	-0.06	(0.11)
1993 data	-0.437**	(0.040)	-1.867**	(0.204)	-0.11	(0.11)	N/A	
1994 data	-0.373**	(0.037)	-1.44**	(0.18)	-0.05	(0.10)	N/A	
1995 data	-0.317**	(0.034)	-1.16**	(0.15)	-0.096	(0.056)	N/A	

* = statistically significant at the 0.05 level; ** = statistically significant at the 0.01 level.
X^{L} = Variable lagged by one year

Table 6.3. Predicted Probability of HMO membership

	Big Panel IV	Big Panel DFM	Small Panel IV	Cross-sect'l IV
Under actual data	12.11%	5.64%	9.07%	56.54%
When "excellent health" is coded "no" for all observations	11.54%	5.34%	8.38%	56.27%
When "excellent health" is coded "yes" for all observations	12.78%	5.95%	9.76%	56.83%
When ADLs is coded 0 for all observations	11.73%	not computed	13.63%	57.06%
When ADLs is coded 6 for all observations	12.17%	not computed	8.70%	56.47%
When IADLs is coded 0 for all observations	11.46%	not computed	4.45%	55.37%
When IADLs is coded 6 for all observations	12.24%	not computed	10.08%	56.76%
When "health limits social life" is coded "no" for all observations	12.34%	not computed	8.90%	56.57%
When "health limits social life" is coded "yes" for all observations	11.64%	not computed	9.51%	56.45%
When "history of arthritis" is coded "no" for all observations	11.71%	5.044%	8.73%	56.27%
When "history of arthritis" is coded "yes" for all observations	12.49%	5.82%	9.30%	56.81%
When "history of cancer" is coded "no" for all observations	12.28%	not computed	9.28%	56.62%
When "history of cancer" is coded "yes" for all observations	11.35%	not computed	8.17%	56.11%
When "history of stroke" is coded "no" for all observations	12.25%	not computed	9.26%	56.56%
When "history of stroke" is coded "yes" for all observations	11.09%	not computed	7.30%	56.35%

6.3.1. Instrumental variables estimation

The coefficients on the health status variables in Table 6.2 indicate the extent to which favorable selection persists in Medicare HMOs. These results suggest that there is some favorable selection, but that it may be a rather weak effect. Four of the five general indicators of health status (poor self-reported health, ADLs, IADLs, and "health limits social life") fail to achieve statistical significance, though their coefficients are all signed in a direction suggesting that good health is associated with

HMO membership. The coefficient for "excellent self-reported health" is positive and statistically significant at the 1 percent level.

The mean predicted probability of HMO membership for the actual data was 12.11 percent. The effect of a particular righthand side variable on the dependent variable can be illustrated by comparing the mean predicted probabilities when all of the observations are recoded to 1 for the X-variable to the average probability when all of the observations for that variable are recoded to 0. When all observations are coded to take on a "no" value for "excellent self-reported health," the mean predicted probability of HMO membership is 11.54 percent. When all observations are coded to "yes" for that variable, the average probability is 12.78 percent. This is an increase of 1.24 percentage points (10.75%). When the ADLs variable is set to its minimum (0) for all observations, the mean predicted probability of HMO membership is 11.73 percent; when it is set to its maximum (6), the mean probability rises to 12.17 percent. When IADLs is set to 0, the mean probability is 11.46 percent ; when it is set to 1, the mean probability is 12.24 percent. When all observations are coded to take on a "no" value for the "health limits social life" variable, the mean predicted probability is 12.34 percent. When all are coded to "yes," the probability drops to 11.64 percent. All of these effects are consistent with favorable selection. The effects are, however, relatively small.

All three of the individual chronic conditions variables are significant at the 5 percent level. Two of them (stroke and cancer) have negative signs, consistent with the favorable selection hypothesis. The other, arthritis, has a positive sign. One explanation for this may be that arthritis sufferers typically require prescription drugs,

and HMOs provide superior coverage for prescription drugs. Stroke and cancer, on the other hand, are likely to require specialist care and hospitalization, which are more easily accessed in a fee-for-service setting. Coding all observations to "no" for the arthritis variable yields an average probability of membership of 11.71 percent; coding them to "yes" causes the probability to increase to 12.49 percent. The predicted mean probability when all observations have "no" values for the history of cancer variable is 12.28 percent; the probability when all have "yes" values drops to 11.35 percent. The mean probability when all observations have "no" values for history of stroke is 12.25 percent, while the mean probability when all have "yes" values is 11.09 percent.

Some of the sociodemographic variables commonly associated with poor health also indicate continued favorable selection. The coefficient on nursing home residence, for example, is negative and statistically significant at the 1 percent level. Additionally, the coefficients on the two age variables indicate that advanced age decreases the likelihood of choosing an HMO ($p < 0.01$). Very elderly persons and persons residing in nursing homes are more likely than the young-elderly and those living in the community to have chronic health problems. Thus, to the extent that they are less likely to join an HMO, this is evidence of favorable selection.

The coefficients on the market penetration interaction terms in this regression indicate whether or not the degree of favorable selection into Medicare HMOs changes according to the degree of HMO market penetration in a county. Of the six interaction terms, only one—market penetration interacted with number of chronic conditions—achieves statistical significance ($p < 0.01$). This coefficient indicates that the presence of high market penetration in a county significantly weakens the positive relationship

between excellent health and HMO choice. But on most dimensions of health status, market penetration does not appear to alter the favorable selection dynamic, contrary to this study's hypothesis.

6.3.2. Discrete factor model

The results of the discrete factor model (DFM) estimation of the HMO choice model are available in Table 6.2. Five mass points were used in the estimation. The values of the first and last mass points were normalized to zero and one, respectively. The values of the remaining three mass points were estimated by the DFM program (results not presented). Note that the DFM program does not report any statistic describing overall model fit.

The results of the DFM estimation are quite consistent with the instrumental variables (IV) results for the big panel dataset. As before, the excellent self-reported health variable is statistically significant and positive, though its significance level dropped to the 5 percent level in the DFM estimation. The average probability of HMO membership when all observations are coded to take on a "no" value for excellent health is 5.34 percent; and when all are coded with a "yes" value for excellent heath the average probability is 5.95 percent (a 11.42 percent change) Poor self-reported health and "health limits social life" remain negative and not significant. IADLs remains positive and not significant. ADLs, however, curiously changes sign from positive to negative in the DFM estimation. The chronic conditions variables for arthritis and stroke remain significant, but cancer is not significant in the DFM results. Having a history of arthritis increases the average probability of HMO membership

from 5.044 percent to 5.82 percent. The results for the age and nursing home residence are the same as in the IV estimation. Overall, the results for the health status indicators are very similar to those of the IV estimation: they provide some support for the favorable selection hypothesis, though not unmitigated support.

The results for the interaction terms are also consistent with the IV estimation. Only market penetration*number of chronic conditions is significant ($p < 0.01$), and the coefficient is negative. There is little in these results to bolster the hypothesis that the amount of favorable selection into Medicare HMOs varies with market penetration.

6.4. Small panel dataset results

The results of the XPROBIT estimation of the HMO equation for the small panel dataset are available in Table 6.2. There are fewer significant coefficients in the small panel regression than in either of the big panel estimations. Only one of the five general health status indicators, IADLs, is statistically significant ($p < 0.05$). "Excellent self-reported health" is not significant, unlike in the big panel models. The average of the predicted probabilities of HMO membership increases from 4.45 percent to 10.08 percent when all observations are coded to take on a zero value for IADLs and then recoded to take on a value of 6.

Consistent with the big panel results, "poor self-reported health," ADLs, and "health limits social life" also are not significant. The signs on these variables, however, are the same as in the big panel regressions (ADLs had a negative coefficient, like in the DFM results but unlike in the big panel IV results). Of the three chronic conditions variables, only stroke is significant in the small panel run ($p < 0.05$).

As with the big panel models, stroke and cancer are negatively signed, while arthritis has a positive sign. The age and nursing home variables, while still negatively signed, are not statistically significant in the small panel regression. Also noteworthy is the fact that none of the interaction terms is statistically significant for the small panel.

Overall, the small panel results are broadly consistent with the big panel results, but significance levels are lower. This is unsurprising given the differences in the datasets: the small panel has a much smaller sample size (7,432 observations compared to 38,185) and does not include persons who died during the year. Differences in the estimation methods used may also account for the differences in the results: the DFM estimates coefficients with greater precision than instrumental variables, and the big panel regressions were not run with sampling weights. The presence of weights in the small panel estimation likely increased the size of the standard errors, pushing the coefficients toward statistical insignificance.

6.5. Cross-sectional dataset results

Table 6.2 reports the results of the IV estimation of the HMO choice model for the cross-sectional dataset. The cross-sectional dataset results were, by and large, consistent with the results of the big panel estimations, except that some significance levels were lower. The results for the "excellent health status," "poor health status," "health limits social life," and IADLs variables were the same in terms of both sign and significance level. Of these, only "excellent health status" achieved statistical significance ($p < 0.01$). When all observations are coded to take on a "no" value for the excellent health variable, the average probability of HMO membership is 56.27

percent; when all are recoded to "yes," the probability rises slightly, to 56.83 percent (a 1 percent change). The coefficient on the ADLS variable has a theoretically peculiar negative sign, like in the big panel DFM estimation. None of the three chronic conditions variables are statistically significant in the cross-sectional estimation, whereas all were significant in the big panel IV estimation. The signs, however, were the same. The cross-sectional results for the market penetration interaction terms are also consistent with the big panel results: again only the penetration * number of chronic conditions variable is significant.

Though there are few inconsistencies between the cross-sectional results and the big panel results, those that do exist may again be explained by both differences in the datasets and differences in the estimation methods. The cross-sectional dataset has 13,373 observations and excludes persons who died; the big panel dataset has 38,185 observations and includes deaths. The cross-sectional model included cross-sectional sampling weights; the big panel estimations were unweighted. Finally, the cross-sectional model estimates differences between HMO and fee-for-service persons at a single point in time, while the panel models measure change over time. These two effects can be quite different.

7. Results: Effect of HMOs on utilization

7.1. Big panel dataset results

7.1.1. Naive model

A naïve model was run in order to provide baseline estimates from which to judge the gains associated with a simultaneous equations modeling approach. The naïve model consisted of random effects probit and OLS (XTPROBIT and XTGEE) estimations with Huber/White corrections to the standard errors. That is, the naïve model accounted for the panel data structure but did not account for the endogeneity of the HMO variable.

Since the purpose of each of the three utilization equations was to test the effect of the HMO practice setting on utilization, controlling for confounding variables, the primary result of interest is the coefficient on the endogenous HMO variable. A negative and statistically significant coefficient would indicate that HMOs are able to significantly reduce health services utilization. The naïve model results for both the probability of hospitalization equation and the number of inpatient days equation show precisely this effect. For the probability of hospitalization model, the coefficient on the HMO variable is –0.511 and the standard error is 0.037 ($p<0.001$). For the inpatient days equation, $\beta=-0.212$ and s.e.$=0.070$ ($p=0.002$). The HMO variable is also statistically significant in the physician visits model, but its sign is positive ($\beta=0.245$, s.e.$=0.036$, $p<0.001$). This means that HMO membership significantly increases the probability of having at least one physician visit during the year.

It is important to bear in mind that these results derive from a model which does not control for the endogeneity between HMO membership and utilization. Thus, the error term of this model includes unmeasured aspects of health status that affect both the likelihood of HMO membership and utilization. This creates bias in the estimates in the utilization equation because it means that the HMO variable is correlated with the error term. Consequently, it is necessary to use simultaneous equations to obtain unbiased estimates of the HMO effect.

7.1.2. Instrumental variables estimation

7.1.2.1. Model diagnostics

Several specification tests were performed on the utilization models. A Wald test, already discussed in the context of the HMO equation in Section 6, was used to ascertain whether the 16 chronic conditions variables belonged in the utilization equations. For the big panel dataset, the variables were found to be jointly significant for the probability of hospitalization model ($\chi^2(16) = 647.63$, $p<0.01$), as well as the inpatient days model ($\chi^2(16) = 52.31$, $p<0.01$) and the physician visits model ($\chi^2(16) = 624.64$, $p<0.01$). Consequently, the 16 variables were retained in all three utilization equations.

A modified version of a test described by Norton and Staiger (1994) was used to test whether the market penetration variables collectively constitute a valid identifying restriction in this system of simultaneous equations. Norton and Staiger's test examines whether the identifying restrictions are correlated with the endogenous variable—here, whether the market penetration variables are correlated with the HMO

choice variable. The first equation in the system is run including the group of instruments, and their joint significance is examined. Norton and Staiger use a likelihood ratio test to determine their joint significance, but for the reasons discussed earlier, in this analysis a Wald test was used.

The Norton and Staiger test determined that for the big panel dataset, the nine market penetration variables (including the 6 interaction terms) were jointly significant in the HMO choice equation ($\chi^2(9) = 547.15$, $p<0.01$), indicating that they are good instruments. This test was rerun on just the three market penetration main effects (not the interactions) and the result was that even by themselves, these variables jointly were good instruments ($\chi^2(3) = 67.58$, $p<0.01$).

Norton has proposed a second test for determining whether identifying restrictions are valid: In a two-equation system, the second equation is run including actual (not predicted) values of the endogenous variable plus all of the instruments, and a Wald test is performed on the instruments. If the instruments are jointly significant, this indicates that they are not good instruments. The theoretical rationale is that a good instrument is a variable that is significantly related to the endogenous variable but is not a significant predictor of the dependent variable in the second equation.

In the present analysis, each of the three utilization equations was run according to Norton's procedure. Again, the nine instruments (including interaction terms) were tested collectively, and then a second test was performed of the three main effects instruments only. For the big panel dataset, the following results were obtained:

Table 7.1. Norton Test Results - Big Panel Dataset[1]

	Including interactions	Excluding interactions
Pr(hospitalization) model	Not jointly significant ($\chi^2(9) = 14.18$, p = 0.1160)	Not jointly significant ($\chi^2(3) = 1.07$, p = 0.7837)
Inpatient days model	Jointly significant ($\chi^2(9) = 27.38$, p = 0.0012)	Jointly significant ($\chi^2(3) = 11.46$, p = 0.0095)
Physician visits model	Not jointly significant ($\chi^2(9) = 11.76$, p = 0.2270)	Jointly significant ($\chi^2(3) = 9.22$, p = 0.0265)

[1] "Not jointly significant" indicates variables are good instruments

These results indicate that the nine instruments collectively constitute good instruments for the probability of hospitalization equation and the physician visits equation, but not for the inpatient days model. When the interaction terms are excluded from the test, the remaining three variables are only valid instruments for the probability of hospitalization model.

It is difficult to account for why the market penetration indicators were found collectively to be a significant predictor of the number of hospital days, but not the probability of hospitalization. The signs of the coefficients on these variables differed in a way that is not theoretically explicable: The "percent penetration" variable had a negative and statistically significant coefficient ($\beta = -1.24$, $p < 0.01$), while the "number of Medicare HMOs" variable had a positive and statistically significant coefficient ($\beta = 0.00303$, $p < 0.01$). Only two of the six interaction terms were individually significant at the 0.05 level, and the direction of the effects of these terms were also inconsistent. The three interaction terms indicating poor health all had positive signs, and two of the three variables indicating good health also had positive signs. In short, although the Norton test indicates that these variables jointly are significant predictors

of inpatient days, no clear theoretical rationale for this result emerges. The issue is complicated by the fact that for the small panel dataset, these variables were not jointly significant for the inpatient days model –that is, they were found to be good instruments. The results for the big panel dataset remain a cause for concern, however.

A test described by Pindyck and Rubinfeld (1981) was used to test the exogeneity of the HMO choice variable. A finding that this variable is exogenous indicates that the utilization equations may appropriately be estimated using simply probit and OLS regressions; a finding that it is endogenous means that simultaneous equations methods are needed. The test procedure is as follows: (1) the HMO variable is regressed on all the predetermined variables in the entire system; (2) the utilization variable is regressed on all the predetermined variables in the utilization equation plus the HMO variable and predicted values of the HMO variables obtained in Step 1; and (3) the coefficient on the predicted HMO variable is examined. The null hypothesis is exogeneity; thus, a statistically significant coefficient indicates that the HMO variable is endogenous.

The results of the Pindyck and Rubinfeld test were quite unexpected. Contrary to all theory, the HMO choice variable was found not to be endogenous for any of the utilization models using the big panel dataset. For the probability of hospitalization model, the coefficient on the predicted HMO variable was 0.061 and the p-value was 0.458. For the inpatient days model, β=0.265, p=0.065. For the physician visits model, β=-0.078 and p=0.367. There is no easy explanation for these results.

Theory, as well as empirical findings in the literature to date, strongly suggest that endogeneity exists between HMO choice and subsequent health services utilization. This issue is discussed in greater depth in Section 8.

7.1.2.2. Regression results

Table 7.2 through 7.4 summarize the results of the utilization models for all four estimations (big panel instrumental variables, big panel discrete factor, small panel instrumental variables, and cross-sectional). Predicted probabilities for each of the utilization equations are reported in Tables 7.5 through 7.7. A discussion of each of the equations for each of the estimations follows.

Table 7.2. Probability of Hospitalization

	Big Panel IV (xtprobit) n=38,185 χ^2_{60}=4045.10 (p=0.0000)		Big Panel DFM (logit) n=38,185		Small Panel IV (xtprobit) n=7,384 χ^2_{60}=604.19 (p=0.0000)		Cross-Sectional (probit) n=13,373 χ^2_{56}=1274.58 (p=0.0000) Pseudo-R^2=0.1300	
	Coeff.	Robust s.e.	Coeff.	Robust s.e.	Coeff.	Robust s.e.	Coeff.	Robust s.e.
Constant	-1.441**	(0.074)	-2.26**	(0.18)	-1.32**	(0.23)	-1.24**	(0.14)
HMO membership	-0.460**	(0.068)	-1.146**	(0.089)	-0.67**	(0.23)	-0.55**	(0.13)
Health and functional status:								
Excellent health	-0.170 **	(0.020)	-0.319**	(0.035)	-0.135*	(0.056)	-0.153**	(0.038)
Poor health	0.200**	(0.029)	0.328**	(0.048)	0.164	(0.088)	0.190**	(0.058)
ADLs	-0.0278**	(0.0079)	-0.046**	(0.013)	-0.310	(0.025)	-0.036*	(0.015)
IADLs	-0.0513**	(0.0077)	-0.088**	(0.012)	-0.074**	(0.024)	-0.071**	(0.015)
Health limitations on social life	0.311**	(0.020)	0.547**	(0.033)	0.327**	(0.059)	0.296**	(0.038)
History of arthritis	-0.018	(0.018)	-0.024	(0.031)	-0.023	(0.056)	-0.016	(0.034)
History of rheumatoid arthritis	-0.028	(0.025)	-0.056	(0.040)	-0.058	(0.070)	-0.024	(0.049)
History of atherosclerosis	-0.015	(0.023)	-0.039	(0.038)	-0.165*	(0.069)	0.019	(0.047)
History of broken hip	0.171**	(0.033)	0.2878**	(0.0508)	0.146	(0.094)	0.1796**	(0.0602)
History of cancer	0.139**	(0.021)	0.227**	(0.034)	0.208**	(0.065)	0.211**	(0.039)
History of coronary heart disease	0.069**	(0.024)	0.119**	(0.039)	0.130600	(0.070005)	0.040	(0.047)
History of skin cancer	-0.069**	(0.023)	-0.124**	(0.039)	-0.010	(0.065)	-0.096*	(0.044)
History of diabetes	0.108**	(0.022)	0.191**	(0.036)	0.075	(0.069)	0.052	(0.043)
History of emphysema/COPD	0.124**	(0.024)	0.207**	(0.038)	0.122	(0.068)	0.201**	(0.044)
History of high blood pressure	0.055**	(0.018)	0.0972**	(0.0301)	-0.007	(0.053)	0.050	(0.034)
History of myocardial infarction	0.272**	(0.024)	0.459**	(0.038)	0.368**	(0.071)	0.315	(0.046)
History of osteoporosis	-0.020	(0.027)	-0.039	(0.044)	0.030	(0.088)	0.026	(0.051)
History of other heart problem	0.190**	(0.019)	0.332**	(0.031)	0.223**	(0.056)	0.177**	(0.037)
History of Parkinson's Disease	-0.016	(0.053)	-0.047	(0.086)	0.112	(0.120)	0.074	(0.11)
History of partial paralysis	-0.106**	(0.036)	-0.186**	(0.055)	-0.161	(0.113)	-0.1862**	(0.0707)
History of stroke	0.136**	(0.026)	0.219**	(0.041)	0.099	(0.083)	0.250**	(0.047)
Demographics:								
Age 75 to 84	0.108**	(0.020)	0.192**	(0.034)	0.110	(0.058)	0.103**	(0.036)
Age 85+	0.122**	(0.027)	0.211**	(0.044)	0.107	(0.077)	0.095	(0.049)
Less than high school education	-0.003	(0.022)	-0.008	(0.036)	0.060	(0.068)	-0.016	(0.042)

Some college	-0.058**	(0.022)	-0.103**	(0.039)	-0.135*	(0.064)	-0.061	(0.042)
Medigap policy	0.188**	(0.026)	0.279**	(0.042)	0.045	(0.069)	0.141**	(0.054)
Black	-0.031	(0.032)	-0.077	(0.054)	-0.070	(0.095)	-0.071**	(0.059)
Other nonwhite race	-0.067	(0.059)	-0.150	(0.095)	-0.289	(0.249)	-0.04	(0.11)
Male	0.114**	(0.019)	0.205**	(0.034)	0.175**	(0.057)	0.067	(0.037)
Married	-0.0514*	(0.0204)	-0.096**	(0.035)	-0.052	(0.057)	-0.019	(0.039)
Nursing home resident	0.401**	(0.046)	0.683**	(0.070)	0.272	(0.156)	0.142	(0.110)
Has a usual source of care	0.352**	(0.036)	0.677**	(0.059)	0.370**	(0.106)	0.223**	(0.076)
Income <$10,000	0.030	(0.022)	0.045	(0.038)	-0.001	(0.063)	0.070	(0.045)
Income $30,001-$50,000	-0.007	(0.029)	-0.025	(0.051)	-0.009	(0.091)	0.014	(0.055)
Income >$50,000	-0.066	(0.043)	-0.131	(0.08)	-0.249*	(0.113)	-0.086	(0.084)
Medicaid recipient	0.045	(0.033)	0.046	(0.051)	-0.0460	(0.1004)	0.006	(0.063)
New England division	0.0510	(0.0508)	0.097	(0.087)	0.246	(0.153)	0.110	(0.099)
Mid Atlantic division	0.083**	(0.028)	0.125**	(0.048)	0.094	(0.082)	0.072	(0.054)
East North Central division	0.050	(0.029)	0.081**	(0.049)	0.084	(0.087)	0.008	(0.054)
West North Central division	0.037	(0.039)	0.057	(0.066)	0.062	(0.108)	0.022	(0.078)
East South Central division	0.104*	(0.044)	0.157*	(0.072)	0.260*	(0.129)	0.109	(0.086)
West South Central division	0.082*	(0.033)	0.117*	(0.056)	-0.135*	(0.095)	-0.001	(0.062)
Mountain division	0.066	(0.041)	0.092	(0.069)	-0.160	(0.121)	0.088	(0.075)
Pacific division	0.035	(0.034)	0.043	(0.058)	-0.006	(0.108)	0.106	(0.066)
Large fringe metro county	0.087	(0.049)	0.156	(0.080)	0.191	(0.126)	0.018	(0.092)
Medium metro county	-0.037	(0.027)	-0.071	(0.045)	0.058	(0.081)	0.063	(0.052)
Lesser metro county	-0.010*	(0.039)	-0.178**	(0.067)	-0.064	(0.115)	-0.014	(0.082)
Urbanized nonmetro county	-0.001	(0.042)	-0.00024	(0.07)	-0.029	(0.118)	0.124	(0.078)
Less urbanized nonmetro county	0.024	(0.037)	0.045	(0.063)	0.192	(0.106)	0.1005	(0.0705)
Thinly populated county	-0.006	(0.105)	-0.05	(0.17)	0.299	(0.225)	-0.229	(0.190)
Control variables:								
Physician-to-population ratio	-2.63	(7.66)	-6.30	(12.98)	11.33	(20.41)	8.35	(14.74)
Hospital-bed-to-population ratio	7.77	(4.27)	14.06*	(6.94)	-0.96	(11.41)	4.650	(7.803)
AAPCC rate	0.0725**	(0.0001)	0.0006*	(0.0002)	0.0003	(0.0004)	0.00052*	(0.00024)
Missing education data	-0.142**	(0.053)	-0.214*	(0.083)	-0.605**	(0.230)	-0.013	(0.092)
Missing Medigap data	-0.159*	(0.064)	-0.305**	(0.105)	-0.284	(0.207)	not included	
Missing income data	0.024	(0.058)	0.054	(0.099)	-0.493	(0.280)	0.055	(0.073)
1993 data	0.073**	(0.026)	0.12**	(0.046)	0.028	(0.078)	not included	
1994 data	0.031	(0.024)	0.039	(0.043)	0.017	(0.065)	not included	
1995 data	-1.441*	(0.074)	0.0655	(0.0404)	-1.32**	(0.23)	not included	

* = statistically significant at the 0.05 level; ** = statistically significant at the 0.01 level.

Table 7.3. Number of Inpatient Days[1]

	Big Panel IV (xtgee) n=7,338 χ^2_{60}=1407.91 (p=0.0000)		Big Panel DFM n=7,338		Small Panel IV (xtgee) n=1,169 χ^2_{60}=308.96 (p=0.0000)		Cross-Sectional (OLS) n=2,171 $F_{56,2114}$=8.16 (p=0.0000) R^2=0.1954	
	Coeff.	Robust s.e.	Coeff.	Robust s.e.	Coeff.	Robust s.e.	Coeff.	Robust s.e.
Constant	-1.690**	(0.104)	2.53**	(0.14)	2.04**	(0.29)	1.93**	(0.23)
HMO membership	-0.01	(0.12)	-0.696**	(0.077)	-0.01	(0.33)	-0.41*	(0.21)
Health and functional status:								
Excellent health	-0.059	(0.032)	-0.038	(0.032)	-0.071	(0.084)	-0.021	(0.057)
Poor health	0.211**	(0.037)	0.212**	(0.037)	0.11	(0.12)	0.135	(0.073)
ADLs	-0.0018	(0.0102)	-0.001	(0.010)	-0.022	(0.032)	0.002	(0.019)
IADLs	-0.0851**	(0.0102)	-0.0853**	(0.0101)	-0.1019**	(0.0305)	-0.119**	(0.019)
Health limitations on social life	0.159**	(0.028)	0.165**	(0.028)	0.102	(0.070)	0.210**	(0.052)
History of arthritis	-0.049	(0.028)	-0.044	(0.027)	-0.080	(0.086)	0.040	(0.049)
History of rheumatoid arthritis	-0.005	(0.035)	-0.023	(0.034)	0.111	(0.089)	-0.090	(0.065)
History of atherosclerosis	-0.029	(0.032)	-0.037	(0.032)	-0.091	(0.084)	0.005	(0.063)
History of broken hip	0.059	(0.040)	0.066	(0.041)	0.104	(0.111)	0.054	(0.072)
History of cancer	0.050	(0.029)	0.038	(0.029)	0.064	(0.082)	0.053	(0.053)
History of coronary heart disease	-0.038	(0.033)	-0.034	(0.032)	-0.098	(0.080)	0.067	(0.063)
History of skin cancer	-0.039	(0.035)	-0.052	(0.034)	-0.0882	(0.0902)	-0.096	(0.061)
History of diabetes	0.122**	(0.032)	0.104**	(0.031)	-0.105	(0.097)	0.099	(0.061)
History of emphysema/COPD	0.135**	(0.032)	0.128**	(0.032)	0.270**	(0.085)	0.043	(0.058)
History of high blood pressure	0.005	(0.027)	-0.01	(0.026)	0.069	(0.080)	-0.010	(0.047)
History of myocardial infarction	0.073*	(0.032)	0.074*	(0.031)	0.076	(0.094)	0.142*	(0.057)
History of osteoporosis	0.032	(0.037)	0.028	(0.038)	0.066	(0.105)	-0.032	(0.063)
History of other heart problem	0.004	(0.027)	-0.004	(0.027)	-0.087	(0.105)	0.005	(0.050)
History of Parkinson's Disease	-0.072	(0.070)	-0.074	(0.070)	-0.333	(0.221)	0.105	(0.106)
History of partial paralysis	0.035	(0.045)	0.030	(0.045)	0.1155	(0.1402)	-0.033	(0.099)
History of stroke	-0.005	(0.034)	-0.008	(0.033)	-0.090	(0.094)	0.104	(0.063)
Demographics:								
Age 75 to 84	0.052	(0.031)	0.035	(0.0306)	-0.001	(0.085)	0.022	(0.054)
Age 85+	-0.068	(0.039)	-0.098**	(0.038)	-0.097	(0.107)	-0.224**	(0.067)
Less than high school education	0.042	(0.032)	0.036	(0.031)	0.044	(0.095)	0.0002	(0.0563)

Some college	-0.001	(0.036)	-0.010	(0.035)	-0.0981	(0.1004)	-0.021	(0.060)
Medigap policy	0.130**	(0.039)	0.026	(0.035)	0.220*	(0.098)	0.053	(0.072)
Black	0.163**	(0.048)	0.152**	(0.047)	0.17	(0.16)	0.058	(0.095)
Other nonwhite race	-0.086	(0.095)	-0.093	(0.084)	0.29	(0.25)	-0.32*	(0.15)
Male	0.108**	(0.031)	0.117**	(0.030)	0.092	(0.088)	0.028	(0.055)
Married	-0.118**	(0.031)	0.1117**	(0.0306)	-0.155	(0.082)	0.046	(0.056)
Nursing home resident	0.401**	(0.052)	0.397**	(0.050)	0.26	(0.17)	-0.01	(0.17)
Has a usual source of care	0.035	(0.050)	0.068	(0.048)	-0.18	(0.15)	-0.22	(0.15)
Income <$10,000	0.067*	(0.033)	0.075*	(0.033)	0.113	(0.085)	0.1316*	(0.0605)
Income $30,001-$50,000	-0.062	(0.047)	-0.084	(0.046)	0.04	(0.12)	-0.005	(0.076)
Income >$50,000	-0.146	(0.079)	-0.163*	(0.074)	0.12	(0.19)	-0.24	(0.15)
Medicaid recipient	-0.077	(0.044)	-0.155**	(0.043)	0.11	(0.12)	-0.023	(0.081)
New England division	0.187*	(0.082)	0.191*	(0.076)	0.14	(0.19)	0.07	(0.13)
Mid Atlantic division	0.246**	(0.044)	0.206**	(0.042)	0.16	(0.12)	0.137	(0.080)
East North Central division	0.014	(0.042)	-0.005	(0.043)	0.054	(0.108)	0.066	(0.075)
West North Central division	-0.072	(0.059)	-0.052	(0.058)	0.15	(0.15)	-0.12	(0.11)
East South Central division	0.003	(0.063)	-0.016	(0.063)	0.14	(0.20)	-0.10	(0.12)
West South Central division	0.107*	(0.049)	0.096*	(0.049)	0.07	(0.12)	0.126	(0.085)
Mountain division	-0.113	(0.068)	0.002	(0.061)	-0.08	(0.21)	-0.043	(0.12)
Pacific division	-0.187**	(0.054)	-0.093	(0.051)	-0.01	(0.13)	-0.051	(0.099)
Large fringe metro county	-0.042	(0.073)	-0.064	(0.069)	-0.004	(0.151)	0.21	(0.13)
Medium metro county	0.0852*	(0.0402)	0.055	(0.039)	0.0476	(0.1009)	0.074	(0.071)
Lesser metro county	-0.097	(0.058)	-0.13*	(0.06)	0.03	(0.16)	-0.02	(0.11)
Urbanized nonmetro county	0.178**	(0.060)	0.121*	(0.061)	0.44**	(0.15)	0.242*	(0.107)
Less urbanized nonmetro county	-0.018	(0.056)	-0.072	(0.055)	0.05	(0.14)	-0.015	(0.095)
Thinly populated county	-0.354*	(0.142)	-0.42**	(0.15)	0.47	(0.24)	-0.22	(0.29)
Control variables:								
Physician-to-population ratio	-6.42	(11.59)	-6.89	(11.16)	6.51	(30.33)	20.01	(21.085)
Hospital-bed-to-population ratio	-4.74	(5.69)	-5.71	(5.83)	1.57	(15.73)	15.44	(11.46)
AAPCC rate	0.0009**	(0.0002)	0.0011**	(0.0002)	0.00144*	(0.00057)	0.00094**	(0.00035)
Missing education data	-0.2629**	(0.0705)	-0.277**	(0.072)	0.26	(0.26)	-0.11	(0.13)
Missing Medigap data	-0.086	(0.089)	-0.132	(0.085)	-0.14	(0.27)	not included	
Missing income data	-0.051	(0.086)	-0.073	(0.086)	-0.54	(0.35)	-0.002	(0.105)
1993 data	0.381**	(0.041)	0.2892**	(0.0406)	0.28*	(0.12)	not included	
1994 data	0.293**	(0.038)	0.224**	(0.038)	0.073	(0.099)	not included	
1995 data	0.102**	(0.034)	0.049	(0.035)	0.055	(0.079)	not included	

* = statistically significant at the 0.05 level; ** = statistically significant at the 0.01 level.

[1] Dependent variable was ln(inpatient days + constant). Sample consisted of those individuals with at least one hospitalization.

Table 7.4. Probability of Physician Visit

	Big Panel IV (xtprobit) n=38,003[1] χ^2_{60}=6498.06 (p=0.0000)		Big Panel DFM (logit) n=38,003[1]		Small Panel IV (xtprobit) n=7,392[1] χ^2_{47}=897.30 (p=0.0000)		Cross-Sectional (probit) n=13,327[1] χ^2_{55}=2847.28 (p=0.0000) Pseudo-R^2=0.36	
	Coeff.	Robust s.e.	Coeff.	Robust s.e.	Coeff.	Robust s.e.	Coeff.	Robust s.e.
Constant	-0.674**	(0.084)	-5.36**	(0.29)	-0.14	(0.26)	-1.743**	(0.142)
HMO membership	0.125	(0.070)	2.37**	(0.11)	-0.15	(0.26)	0.10	(0.14)
Health and functional status:								
Excellent health	-0.1300**	(0.0201)	-0.314**	(0.048)	-0.2989**	(0.0610)	-0.174**	(0.044)
Poor health	-0.146**	(0.040)	-0.325**	(0.088)	0.21	(0.14)	0.136	(0.085)
ADLs	0.0371**	(0.0095)	0.082**	(0.022)	0.0492	(0.0308)	0.046**	(0.017)
IADLs	0.0542**	(0.0090)	0.1183**	(0.0206)	0.018	(0.032)	0.136**	(0.016)
Health limitations on social life	0.0875**	(0.023)	0.225**	(0.054)	0.158*	(0.063)	0.293**	(0.049)
History of arthritis	0.2453**	(0.0203)	0.531**	(0.046)	0.310**	(0.063)	0.247**	(0.040)
History of rheumatoid arthritis	0.116**	(0.032)	0.289**	(0.068)	not included		0.2497**	(0.0702)
History of atherosclerosis	0.0297	(0.0304)	0.068	(0.068)	not included		0.004	(0.058)
History of broken hip	-0.026	(0.044)	-0.046	(0.094)	not included		-0.064	(0.076)
History of cancer	0.115**	(0.026)	0.288**	(0.059)	0.360**	(0.096)	0.218**	(0.053)
History of coronary heart disease	0.113**	(0.032)	0.27**	(0.072)	not included		0.155**	(0.057)
History of skin cancer	0.165**	(0.029)	0.381**	(0.062)	not included		0.334**	(0.056)
History of diabetes	0.154**	(0.029)	0.363**	(0.065)	not included		0.3104**	(0.0601)
History of emphysema/COPD	0.144**	(0.030)	0.354**	(0.067)	not included		0.257**	(0.062)
History of high blood pressure	0.274**	(0.021)	0.614**	(0.045)	not included		0.307**	(0.040)
History of myocardial infarction	-0.016	(0.032)	-0.061	(0.069)	not included		0.095	(0.061)
History of osteoporosis	0.135**	(0.035)	0.323**	(0.079)	not included		0.158*	(0.071)
History of other heart problem	0.130**	(0.023)	0.293**	(0.054)	not included		0.133**	(0.044)
History of Parkinson's Disease	0.152	(0.085)	0.36*	(0.17)	not included		0.351**	(0.121)
History of partial paralysis	-0.051	(0.047)	-0.08	(0.098)	not included		0.020	(0.085)
History of stroke	-0.047	(0.034)	-0.049	(0.075)	0.116	(0.104)	-0.076	(0.060)
Demographics:								
Age 75 to 84	0.033	(0.022)	0.107*	(0.049)	0.090	(0.075)	0.092*	(0.042)
Age 85+	-0.045	(0.031)	-0.047	(0.068)	-0.083	(0.096)	0.025	(0.053)
Less than high school education	-0.018	(0.026)	0.02	(0.056)	0.081	(0.083)	-0.041	(0.050)

Some college	0.088**	(0.026)	0.223**	(0.057)	0.261**	(0.087)	0.053	(0.048)
Medigap policy	0.299**	(0.028)	1.2008**	(0.0602)	0.182*	(0.076)	0.462**	(0.060)
Black	-0.257**	(0.035)	-0.484**	(0.076)	-0.396**	(0.108)	-0.2897**	(0.0701)
Other nonwhite race	-0.152**	(0.058)	-0.24	(0.12)	-0.58**	(0.19)	-0.25*	(0.11)
Male	-0.105**	(0.023)	-0.239**	(0.049)	-0.118	(0.071)	-0.150**	(0.044)
Married	0.061**	(0.023)	0.096	(0.051)	0.014	(0.079)	0.041	(0.045)
Nursing home resident	-2.184**	(0.065)	-4.64**	(0.14)	-2.32**	(0.19)	not included	
Has a usual source of care	1.100**	(0.029)	2.225**	(0.066)	1.12**	(0.082)	1.574**	(0.054)
Income <$10,000	-0.072**	(0.026)	-0.173**	(0.056)	0.013	(0.088)	-0.069	(0.054)
Income $30,001-$50,000	0.069*	(0.032)	0.188**	(0.072)	0.026	(0.102)	0.081	(0.066)
Income >$50,000	0.105*	(0.048)	0.305**	(0.108)	-0.34*	(0.16)	0.318**	(0.101)
Medicaid recipient	0.238**	(0.039)	0.929**	(0.086)	0.02	(0.13)	0.192**	(0.071)
New England division	-0.119*	(0.060)	-0.17	(0.14)	0.11	(0.21)	-0.16	(0.11)
Mid Atlantic division	-0.162**	(0.034)	-0.191*	(0.075)	-0.32**	(0.11)	-0.116	(0.064)
East North Central division	-0.074*	(0.035)	-0.027	(0.079)	-0.14	(0.11)	-0.176**	(0.068)
West North Central division	0.009	(0.049)	-0.066	(0.109)	-0.03	(0.15)	-0.143	(0.089)
East South Central division	-0.134*	(0.053)	-0.19	(0.12)	-0.32*	(0.16)	-0.097	(0.107)
West South Central division	-0.125**	(0.039)	-0.24**	(0.086)	-0.12	(0.13)	-0.172*	(0.072)
Mountain division	-0.163**	(0.046)	-0.753**	(0.099)	-0.02	(0.13)	-0.074	(0.086)
Pacific division	-0.089*	(0.040)	-0.559**	(0.086)	-0.04	(0.13)	-0.046	(0.076)
Large fringe metro county	0.054	(0.061)	0.32*	(0.13)	0.11	(0.21)	-0.03	(0.13)
Medium metro county	-0.004	(0.031)	0.02	(0.07)	0.04	(0.10)	-0.055	(0.061)
Lesser metro county	0.064	(0.049)	0.312**	(0.108)	0.39*	(0.16)	-0.090	(0.093)
Urbanized nonmetro county	-0.075	(0.049)	0.16	(0.11)	0.06	(0.16)	-0.113	(0.096)
Less urbanized nonmetro county	-0.072	(0.044)	0.131	(0.099)	-0.03	(0.15)	-0.222**	(0.086)
Thinly populated county	0.278*	(0.133)	0.70*	(0.28)	-0.38*	(0.34)	0.39	(0.22)
Control variables:								
Physician-to-population ratio	-25.85**	(8.98)	-48.02*	(19.45)	-10.53	(25.99)	-43.829**	(17.058)
Hospital-bed-to-population ratio	-0.79	(4.94)	6.37	(11.33)	-25.79	(14.73)	5.526	(9.403)
AAPCC rate	0.0005**	(0.0002)	-0.00022	(0.00030)	0.00103	(0.00055)	0.00048	(0.00029)
Missing education data	-0.120	(0.084)	-0.29	(0.20)	0.16	(0.24)	-0.557**	(0.090)
Missing Medigap data	-0.626**	(0.169)	-1.09*	(0.43)	-0.32	(0.25)	not included	
Missing income data	-0.117	(0.067)	-0.30	(0.18)	-0.45	(0.26)	-0.3566**	(0.0709)
1993 data	-0.102**	(0.029)	-0.013	(0.066)	0.034	(0.087)	not included	
1994 data	-0.177**	(0.026)	-0.2003**	(0.0602)	-0.011	(0.069)	not included	
1995 data	-0.205**	(0.023)	-0.258**	(0.055)	-0.017	(0.052)	not included	

* = statistically significant at the 0.05 level; ** = statistically significant at the 0.01 level.

[1] Missing data on the dependent variable: Big panel—182 observations; Small panel—40 observations; Cross-sectional—46 observations.

Table 7.5. Mean Predicted Probability of Hospitalization

	Big Panel IV	Big Panel DFM	Small Panel IV	Cross-sect'l IV
Under actual data	19.31%	17.72%	15.64%	56.50%
When HMO variable is coded "no" for all observations	20.36%	18.76%	16.68%	65.48%
When HMO variable is coded "yes" for all observations	10.90%	7.58%	6.12%	52.39%

Table 7.6. Mean Predicted Number of Inpatient Days

	Big Panel IV	Big Panel DFM	Small Panel IV	Cross-sect'l IV
Under actual data	13.267	13.339	10.220	10.086
When HMO variable is coded "no" for all observations	13.530	13.445	10.362	10.654
When HMO variable is coded "yes" for all observations	7.644	7.569	6.911	5.243

Table 7.7. Mean Predicted Probability of Physician Visit

	Big Panel IV	Big Panel DFM	Small Panel IV	Cross-sect'l IV
Under actual data	78.58%	77.68%	82.73%	78.91%
When HMO variable is coded "no" for all observations	78.28%	74.89%	82.94%	78.12%
When HMO variable is coded "yes" for all observations	80.40%	88.99%	80.41%	79.44%

The big panel instrumental variables analysis revealed that HMO membership has a highly significant negative effect on the probability of hospitalization—the same result as in the naïve model. In the instrumental variables model, the mean predicted probability of hospitalization using the actual data was 19.31 percent. When the value of the HMO variable is set to "no" for all observations, this average probability rises to 20.36 percent. When that variable is set to "yes" for the entire sample, the probability drops markedly, to 10.90 percent. This is a decrease of 9.46 percentage points (46.46%)—quite a substantial effect.

The results for several of the "control" variables in the probability of hospitalization model are of some interest. All of the general health status indicators, and 11 of the 16 chronic conditions variables, were statistically significant at the 1 percent level. All of these variables except for two of the chronic conditions (skin cancer and partial paralysis) were signed in the expected direction—i.e., poor health increased the probability of hospitalization. Both of the advanced age categories were positive and significant at the 1 percent level, indicating that the probability of hospitalization increases as Medicare beneficiaries become more elderly. As anticipated, the two insurance indicators, Medigap and Medicaid, also bore a positive relationship to the probability of hospitalization, though Medicaid was not statistically significant. Interestingly, none of the income categories was significant. One possible reason is that the main components of the theoretical effect of low income on health care utilization, lack of insurance and poor health, were already captured in other variables.

Turning to the inpatient days model, the big panel instrumental variables analysis revealed that for those persons who were hospitalized, HMO membership is associated with a seemingly large decrease in the number of hospital days; however, this difference is not statistically significant. This result is set forth in Table 7.3. It stands in contrast to the finding of a statistically significant effect of HMO membership in the naïve model. Incorporating a Smearing adjustment, in the big panel instrumental variables model, the average predicted average number of inpatient days decreases from 13.53 to 7.64 with a change in HMO status. While not statistically significant,

this represents a 5.89 day (43.50%) reduction in the number of hospital days for those hospitalized.

Of the five general health status indicators, three are statistically significant in the inpatient days model. "Poor self-reported health," "health limits social life," and IADLs are all significant at the 1 percent level and signed such that poor health increases the number of hospital days. "Excellent self-reported health" and ADLs are both negatively signed, as anticipated, but were not statistically significant. Only three of the chronic conditions variables (history of myocardial infarction, history of diabetes, and history of emphysema) are significant. All of these are positively signed, again as expected. While nearly all of the chronic conditions significantly affect the probability of hospitalization, most do not have an appreciable affect on the number of inpatient days for those hospitalized.

Both of the advanced age categories are statistically significant in the probability of hospitalization equation, but neither are significant in the inpatient days model. The signs on these variables, however, are positive in both models, indicating that advanced age increases health services consumption. The signs on the income variables are also consistent with those in the probability of hospitalization model: income less than $10,000 is negatively signed while the two high income categories are positively signed. Only income less than $10,000 has a statistically significant effect, though. The effects of the two insurance indicators, Medigap and Medicaid, are very similar to their effects on probability of hospitalization. Medigap coverage has a positive and statistically significant effect, while Medicaid coverage has a negative but not significant effect.

Turning finally to the probability of physician visit model, the main result of interest is that HMO membership does not have a statistically significant effect on the probability of seeing a doctor, once health status is controlled for. As is described in Table 7.4., the coefficient on the HMO variable was positive but did not achieve statistical significance. This is again a departure from the results of the naïve model, which showed HMO membership to have a statistically significant positive impact on the probability of a physician visit.

The average predicted probability of seeing a physician under the actual data was 78.58 percent. When the HMO membership variable was coded to "no" for all observations, this average probability decreased slightly, to 78.28 percent. When all observations were given a "yes" value on the HMO variable, the mean probability increased to 80.40 percent, a change of 2.12 percentage points (2.71%).

All five of the general health status control variables are significant at the 1 percent level and signed in a direction indicating that poor health increases the probability of seeing a doctor. Ten of the 16 chronic conditions variables are also statistically significant at the 1 percent level, and positively signed. None of the age variables attained statistical significance. Both Medigap and Medicaid are positively signed and significant at the 1 percent level, in accordance with theory. Finally, all three of the income categories are significant. The low income category (less than $10,000) has a negative impact on the probability of hospitalization, while the two high income categories have a positive effect. This too, comports with the theory of health services utilization.

These instrumental variables results can be compared to the results on the same dataset using the discrete factor model in order to test their robustness to changes in assumptions about the error distribution.

7.1.3. Discrete factor model

The coefficients for the DFM results in Tables 7.2 and 7.4 are interpretable in the same way as regular logit coefficients; those in Table 7.6 may be interpreted in the same way as regular OLS coefficients. However, computing predicted probabilities requires a special procedure for DFM results.

With respect to the HMO membership variable, the main findings can be summarized as follows: HMO membership has a negative and statistically significant effect on the probability of hospitalization, just as it did in the instrumental variables estimation. HMO membership also significantly decreases the number of inpatient days for those hospitalized. This result is at odds with the insignificance of the HMO variable in the instrumental variables estimation. In the DFM, HMO membership also has a significant effect on the probability of seeing a physician—this time a positive effect. In contrast, the HMO variable was not significant in the physician visit model estimated through the instrumental variables method. Thus, the DFM found that Medicare HMOs have significant impacts on all three kinds of health care utilization, while the instrumental variables estimation only identified a significant effect on the probability of hospitalization.

As shown in Table 7.2, in the DFM probability of hospitalization model, the HMO variable has a negative coefficient that was significant at the 1 percent level. When all observations are recoded to take on a "no" value for the HMO membership variable, the average of the predicted probabilities is 18.76 percent. When all are recoded to "yes," the probability decreases to 7.58 percent, a decrease of 11.18 percentage points (59.59%).

The DFM results for the health status variables are identical to those of the instrumental variables estimation, in terms of both coefficient sign and significance level. All of the general health status indicators, and 11 of the 16 chronic conditions variables, were statistically significant at the 1 percent level. All of those variables except two of the chronic conditions variables were signed such that poor health increases the probability of hospitalization, in accordance with theory. The results for age, Medigap, Medicaid, and income are also precisely the same in the DFM and IV estimations. These results, too, comport with the theory of health services utilization.

The DFM results for the conditional inpatient days model are presented in Table 7.3. HMO membership significantly decreases the number of hospital days for those hospitalized: the coefficient is negative and significant at the 1 percent level. The mean predicted number of hospital days for those hospitalized is 13.34. The mean when all observations are coded with a "no" value for HMO membership is 13.45; when all are coded with a "yes" value, the mean drops to 7.57 days. This is a reduction of 5.88 days, or 43.70 percent.

The DFM and IV methods produce identical results with respect to the health status variables. Poor health, "health limits social life," and IADLs are statistically

significant, while excellent health and ADLs are not. Of the 16 chronic conditions, only myocardial infarction, diabetes, and emphysema have significant effects. There is some divergence in the DFM and IV results for other control variables. Very old age and Medicaid coverage are significant in the DFM but not in the IV estimation. Medigap coverage, on the other hand, is significant in the IV model but not in the DFM. While the lowest income category is significant in both estimations, the two high income categories have different significant levels across estimations. In summary, the DFM for inpatient days produced different results on some of the variables, but the findings concerning the primary variable of interest, HMO membership, were broadly consistent with those of the instrumental variables estimation.

Table 7.4 summarizes the DFM results concerning the probability seeing a physician at least once during the year. The key finding is that HMO membership has a positive and statistically significant ($p<0.01$) effect. The average of the predicted probabilities when all observations are coded with a "no" value for the HMO variable is 74.89 percent. When all are given a "yes" value, the probability rises to 88.99percent, an increase of 14.10 percentage points (18.83%). This is a considerably larger effect than that identified by the IV estimation (2.12 percentage points (2.71%)).

In all other respects, however, the DFM and IV estimations were nearly identical. As in the IV model, all of the general health status variables were statistically significant at the 1 percent level in the DFM estimation. Ten of the 16 chronic conditions variables were significant (as compared to 11 in the IV model). The results for Medigap coverage, Medicaid coverage, age, and income were also identical

in both sign and significance level. It is only with respect to the key variable of interest that the estimation methods produce somewhat different findings.

7.2. Small panel dataset results

7.2.1. Naive model

In the naïve estimation of the utilization equations for the small panel dataset, the HMO membership variable was only statistically significant in the probability of hospitalization model. The signs of the HMO coefficients in the three utilization equations were the same as in the big panel estimation (negative for the two inpatient equations and positive for the physician visits equation). However, unlike in the big panel estimation, the HMO variable did not achieve statistical significance in the conditional inpatient days model.

In the probability of hospitalization equation on the small panel, the HMO variable was negatively signed and highly statistically significant: β =-0.416 and s.e.=0.107 ($p<0.001$). In the conditional days model, HMO membership was not significant: β=-0.18 and s.e.=16 ($p=0.26$). In the physician visits model, the HMO variable was positive but not significant: β=0.082 and s.e.=0.108 ($p=0.45$).

7.2.2. Instrumental variables estimation

7.2.1.1. Model diagnostics

The diagnostics described in Section 7.1.2.1. were run on the small panel dataset models. The joint contribution of the 16 chronic conditions variables was tested using a Wald test. For the probability of hospitalization model, the variables were jointly

significant ($\chi^2(16) = 122.51$, $p<0.01$). For the inpatient days model, however, the variables were found to be jointly insignificant, though at a borderline level ($\chi^2(16) = 25.56$, $p=0.606$). Despite this finding, a decision was made to retain the variables in the model, since they were significant in the hospital days models for the other two datasets, and since on theoretical grounds they seemed extremely important. It was not possible to test the contribution of the 16 chronic conditions variables for the physician visits model, because of the convergence problem described earlier. Instead, the three variables "history of arthritis," "history of cancer," and "history of stroke" were tested, and determined to be jointly significant ($\chi^2(3) = 37.60$, $p<0.01$). Thus, these three variables were retained in the physician visits model for the small panel dataset.

The Norton and Staiger test determined that for the small panel, the nine market penetration variables were jointly significant in the HMO choice model ($\chi^2(9) = 63.77$, $p<0.01$), indicating that they are good instruments. Even when the interaction terms were not included as instruments, the three remaining instruments were found to be jointly significant ($\chi^2(3) = 11.75$, $p<0.01$).

The Norton test, however, produced less satisfactory results:

Table 7.8. Norton Test Results - Small Panel Dataset[1]

	Including interactions	Excluding interactions
Pr(hospitalization) model	Not jointly significant ($\chi^2(9) = 12.81$, $p = 0.1715$)	Not jointly significant ($\chi^2(3) = 0.81$, $p = 0.8468$)
Inpatient days model	Not jointly significant ($\chi^2(9) = 16.16$, $p = 0.0636$)	Not jointly significant ($\chi^2(3) = 3.51$, $p = 0.3195$)
Physician visits model	Jointly significant ($\chi^2(9) = 23.46$, $p = 0.0052$)	Jointly significant ($\chi^2(3) = 9.22$, $p = 0.0265$)

[1] "Not jointly significant" indicates variables are good instruments

The identifying restrictions, whether viewed as a set of nine variables or a set of three

main effects, were good instruments in the probability of hospitalization model and the inpatient days model, but not in the physician visits model. This is puzzling given that for the big panel dataset, the variables were found to be good instruments for the physician visits model but not for the inpatient days model. As with the big panel findings, the coefficients were of different signs for variables that should, on theoretical grounds, affect the dependent variable in the same direction. That is, the two measures of market penetration had opposite signs, and the measures of good and poor health did not have consistent signs. Only three of the nine variables were individually significant. It is extremely difficult to account for these results.

The Pindyck & Rubinfeld test of the exogeneity of the HMO membership variable failed to identify any endogeneity. As a reminder, a statistically insignificant coefficient on the predicted values of the HMO choice variable indicates that the HMO choice variable is exogenous. In the probability of hospitalization equation, the coefficient on the fitted HMO variable was -0.22 ($p=0.37$). in the inpatient days equation, $\beta=0.40$ and $p=0.38$. In the physician visits equation, $\beta=-0.058$ and $p=0.836$. Consistent with the results for the big panel dataset, the Pindyck & Rubinfeld test for the small panel dataset reveals the surprising results that the HMO choice variable is not endogenous.

As a methodological note, because in the models using the small panel dataset the physician visits equation has a slightly different specification than the other two utilization equations, the first step of the Pindyck & Rubinfeld test was done two different ways. First the HMO variable was regressed on all the predetermined

variables in the two-equation system consisting of the HMO choice equation and the physician visits equation. These results were used to apply the Pindyck & Rubinfeld test to the physician visits model. Then the HMO variable was regressed on all the predetermined variables in the system consisting of the HMO choice equation, the probability of hospitalization equation, and the inpatient days equation. This system has more predetermined variables than the physician visits system for the small panel dataset because it includes all 16 chronic conditions variables. The results of this regression were used to apply the Pindyck & Rubinfeld test to the hospitalization models.

7.2.1.2. Regression results

The results of the instrumental variables estimation for the small panel dataset are set forth in Tables 7.2 through 7.4. In brief, the HMO variable was only statistically significant in the probability of hospitalization model.

As is described in Table 7.2, in the small panel estimation, HMO membership is associated with a statistically significant ($p < 0.01$) decrease in the probability of hospitalization. This finding is consistent with the results of both the big panel IV model and the big panel DFM. In the small panel estimation, the mean predicted probability of hospitalization using the actual data is 15.64 percent. When all observations are given a "no" value for the HMO variable, the mean probability is 16.68 percent; when all are given a "yes value, the mean probability is just 6.12 percent. This is a decrease of 10.56 percentage points (63.31%).

While the signs and significance levels for many of the control variables in the model are the same as in the big panel estimations, there are also a number of differences between the estimations. With respect to the general health status indicators, poor health status and ADLs are not statistically significant in the small panel regression, whereas they are significant at the 1 percent level in the big panel runs. The signs are the same, however. While in the big panel estimations 11 of the 16 chronic conditions variables are significant, in the small panel regression only 4 are. One rather curious finding is that in the small panel model, Medicaid coverage is negatively signed. In the big panel models, it had a positive coefficient. It did not achieve statistical significance in any of the models, though. In summary, the small panel dataset produced results similar to those for the big panel dataset with respect to the HMO variable, but the significance levels of several of the control variables decreased in the small panel run. This is unsurprising for a variety of reasons to be discussed shortly. The most obvious explanation for the difference lies in the much smaller sample size of the small panel.

Moving on to the inpatient days model, Table 7.3 shows that HMO membership does not have a statistically significant impact on the number of inpatient days for those hospitalized. As in the big panel estimations, the coefficient on the HMO variable is negative. Its lack of statistical significance is consistent with the results for the big panel IV estimation, but inconsistent with the DFM results. The Smearing-adjusted average predicted number of inpatient days decreases from 10.36 to 6.91 with a change in the coding of the HMO variable. This represents a decrease of 3.45 days, or 33.30

percent. Despite the magnitude of this drop, the effect did not achieve statistical significance.

With respect to the control variables, there were a number of differences between the small panel results and the big panel results. Poor health and "health limits social life" are not statistically significant in the small panel estimation. Only one of the 16 conditions variables (emphysema) is significant for the small panel (compared to 3 for the big panel). The "75 to 84" age category is negatively signed in the small panel estimation, but positive in the big panel estimations. Finally, while the "less than $10,000" income category is significant in the big panel models, it does not achieve statistical significance for the small panel.

The results for the physician visits equation are reported in Table 7.4. There are two important differences in the probability of physician visit models for the big panel and the small panel datasets. First, there are only 3 chronic conditions variables included in the small panel model, due to the convergence problem discussed earlier. Second, because the small panel sample did not contain any nursing home residents, the nursing home residence variable was dropped from the model for the small panel. Thus, the small panel estimation had a slightly different sample composition and model specification than the big panel. This may account for some of the differences in the results for the big panel and small panel estimations.

In the small panel estimation, the HMO membership variable was not statistically significant. This is the same finding as in the big panel instrumental variables estimation, but in the DFM estimation this variable achieved statistical significance. In the small panel estimation, under the actual data, the average predicted

probability of seeing a physician is 82.73 percent. When all observations are recoded to be fee-for-service enrollees, the mean probability is 82.94 percent; when all are recoded to be HMO enrollees, the mean probability falls to 80.41 percent (a 3.05% drop).

With respect to the control variables in the physician visits model, most of the signs are the same as in the big panel estimations, but far fewer of the variables are statistically significant. Poor health, ADLs, IADLs, and history of stroke all failed to attain statistical significance. Moreover, the Medicaid variable and 2 of the 3 income categories were not significant for the small panel, whereas they were highly significant for the big panel. The signs on the control variables, however, were generally consistent across estimations.

7.3. Cross-sectional dataset results

7.3.1. Naïve model

In the naïve estimation for the cross-sectional dataset, the HMO variable was statistically significant in all three utilization equations. In contrast, in the big panel naïve model, it was significant only in the two inpatient equations; and in the small panel model, it was significant only in the probability of hospitalization equation. The signs on the HMO coefficients, however, were consistent for all of the datasets.

In the cross-sectional regression, HMO membership was negatively signed and significant at the 1 percent level in the probability of hospitalization equation (β =-0.404, s.e.=0.059, $p<0.001$). HMO membership also has a negative and statistically significant effect on the number of hospital days for those hospitalized (β =-0.308,

s.e.=0.109, p=0.005). Finally, in the physician visits model, the HMO variable was positively signed and significant at the 1 percent level (β=0.289, s.e.=0.065, $p<0.001$).

7.3.2. Instrumental variables estimation

7.3.2.1. Model diagnostics

The results of the Wald test (of the 16 chronic conditions variables) were the same for the cross-sectional dataset as for the big panel dataset: in all three utilization equations, the variables were found to be jointly significant. For the probability of hospitalization equation, $\chi^2(16) = 245.60$ and $p<0.01$. For the inpatient days equation, which was estimated through OLS, an F-test rather than a Wald test was used. The F statistic on (16, 2114) degrees of freedom was 1.68 (p=0.0447). For the physician visits equation, $\chi^2(16) = 295.75$ and $p<0.01$. Based on these results, the 16 variables were retained in all three utilization equations.

The Norton and Staiger test results for the cross-sectional dataset indicate that the nine market penetration variables jointly constitute valid instruments for identifying the system ($\chi^2(9) = 174.01$, $p<0.01$), as do the three main effects considered by themselves ($\chi^2(3) = 35.68$, $p<0.01$). However, once again, the Norton test produces mixed results on this question:

Table 7.9. Norton Test Results - Cross-Sectional Dataset[1]

	Including interactions	Excluding interactions
Pr(hospitalization) model	Not jointly significant ($\chi^2(9) = 14.18$, p = 0.1160)	Not jointly significant ($\chi^2(3) = 2.71$, p=0.4388)
Inpatient days model	Jointly significant ($\chi^2(9) = 27.38$, p = 0.0012)	Jointly significant ($\chi^2(3) = 11.46$, p=0.0095)
Physician visits model	Not jointly significant ($\chi^2(9) = 16.35$, p = 0.0599)	Not jointly significant ($\chi^2(3) = 6.57$, p=0.0868)

[1] "Not jointly significant" indicates variables are good instruments

While this test suggests that the identifying restrictions are good instruments for the probability of hospitalization model and the physician visits model, it indicates a problem for the inpatient days model. This is the same set of results as for the big panel dataset. Again, these results are of concern and are not easily interpretable.

As with the other two datasets, the Pindyck & Rubinfeld results for the cross-sectional dataset reveal that the HMO choice variable is not endogenous. For all three utilization equations, the predicted HMO choice variable was not statistically significant. In the probability of hospitalization equation, β =-0.77 and p=0.510. In the inpatient days equation, β =-0.128 and p=0.600. In the physician visits equation, β =-0.077and p=0.653.

7.3.2.2. Regression results

The results of the IV estimation for the cross-sectional dataset are presented in Tables 7.2 through 7.4. The results concerning the impact of HMOs on the probability of hospitalization are broadly consistent with the results from the other estimations: the coefficient on the HMO variables is negative and statistically significant at the 1 percent level. Under the scenario in which all observations are fee-for-service

members, the average probability of hospitalization is 65.48 percent; when all observations are recoded to be HMO enrollees, it drops to 52.39 percent. This represents a 13.09 percentage point (20%) change. While the percentage point change is very close to the percentage point changes found in the panel analyses, in the cross-sectional analysis this represents a much smaller *percent* change (20% versus 46-63% in the panel models), because of the higher average probabilities of hospitalization predicted by the cross-sectional model.

Most of the health status variables in the cross-sectional model have results consistent with those of the big panel regressions. All five of the general health indicators are statistically significant (4 of them at the 1 percent level) and signed such that poor health increases the probability of having a physician visit. Only 7 of the 16 chronic conditions variables are significant, as compared to 11 in the big panel estimations. Some divergence in results can be seen for the sociodemographic control variables. For example, whereas in the big panel estimations both advanced age categories were statistically significant, in the cross-sectional estimation only the "75 to 84" category attained significance. Overall, however, the results of the cross-sectional estimation were very similar to the big panel results.

The cross-sectional results for the inpatient days model, presented in Table 7.3, are quite similar to the results of the big panel estimations, with a few notable departures. The HMO variable has a negative coefficient that is statistically significant at the 5 percent level. This finding of statistical significance is consistent with the results of the big panel DFM estimation, but recall that in the big panel IV estimation, the HMO variable did not achieve statistical significance. In the cross-sectional model,

the Smearing-adjusted average predicted number of inpatient days for individuals with at least one hospitalization is 10.65 when all observations are coded with a "no" value for the HMO variable, and 5.24 when all are coded as HMO members. This represents a 5.41 day (50.79%) decrease.

Most of the general health status indicators had signs and significance levels identical to those of the big panel estimations. The exception was poor health, which lost significance in the cross-sectional estimation. IADLs and "health limits social life" were both significant at the 1 percent level, while ADLs and excellent health were not significant. Of the chronic conditions variables, only history of myocardial infarction was significant. The sociodemographic control variables had results similar to those of the DFM. The highest age category (age 85 and over) was negatively signed and statistically significant; the middle age category (age 75 to 84) was not significant. Medigap coverage had a positive coefficient, but was not significant. Unlike in the DFM, Medicaid coverage also was not significant. Finally, consistent with the big panel results, the lowest income category (under $10,000) was positively signed and statistically significant, while the medium-high income category ($30,000-$50,000) was not significant. The highest income category (over $50,000) lost significance in the cross-sectional model, as compared to the DFM results.

Table 7.4 summarizes the results of the cross-sectional estimation of the physician visits model. HMO membership was found to have a positive but not statistically significant effect on the probability of having seen a physician at least once during the year. This is the same result as in the big panel IV estimation; however, in the DFM, the HMO variable was statistically significant. For the cross-

sectional dataset, HMO membership increases the probability of having a physician visit by less than 1 percent. When all sample members are coded as fee-for-service members, the average predicted probability of seeing a physician is 78.12 percent; when all are recoded to be HMO members, that probability increases to 79.44 percent.

The results for the control variables are very similar to the big panel results. Again, ADLs, IADLs, excellent health, and "health limits social life" are significant and signed such that poorer health increases the probability of seeing a physician. While the poor health variable is also signed in this way, it is not statistically significant. Moreover, in the big panel estimations, poor health had a (rather inexplicable) negative sign. As for the sociodemographic control variables, the highest age category had a different (positive) sign in the cross-sectional estimation than in the big panel estimations, but it was still not significant. The "75 to 84" age category was positive and significant, as in the big panel estimations. Again consistent with the big panel results, Medigap and Medicaid coverage both displayed positive and statistically significant relationships to utilization. Income failed to achieve statistical significance in the cross-sectional model, with the exception of the highest income category.

7.4. Explaining differences in results between the datasets

As is evident from the presentation of regression results, results were not entirely uniform across the three datasets (big panel, small panel, and cross-sectional) and three estimation methods (naïve model, instrumental variables, and discrete factor

model) employed in this study. For the reader's convenience, summaries of the major findings for the different analyses are provided in Tables 7.10 (HMO choice model) and 7.11 (utilization models). A brief discussion of possible reasons for the discrepancies between these findings is in order.

As Tables 7.10 and 7.11 illustrate, a number of the variables' coefficients attained statistical significance in some of the estimations but not in others. Moreover, for a couple of the variables, there were actually differences in the signs on the coefficients. In the HMO choice model, for example, the "health limits social life" variable had a positive sign in the small panel IV estimation, but a negative sign in all other estimations. The interaction of market penetration and the poor health dummy also changed sign depending on the dataset used. In the physician visits model, the primary variable of interest—HMO membership—had a negative sign in one of the estimations (small panel IV) and a positive sign in all other estimations.

Table 7.10. Summary: Effects of Health Status on Probability of HMO Membership

Righthand Side Variable	Big Panel IV (xtprobit)	Big Panel DFM (logit)	Small Panel IV (xtprobit)	Cross-sectional IV (probit)
Excellent health	0.116**	0.44*	0.139	0.110**
Poor health	-0.092	-0.53	-0.63*	-0.26
ADLs	0.0070	-0.012	-0.072	-0.019
IADLs	0.012	0.021	0.115*	0.047
Health limits social life	-0.0654	-0.24	0.06	-0.024
Chronic conditions:				
Arthritis	0.074*	0.28*	0.058	0.104*
Cancer	-0.088*	-0.29	-0.12	-0.099
Stroke	-0.11158*	-0.538**	-0.21*	-0.040
Interaction terms:				
* Excellent health	-0.25	-0.86	-0.56	-0.24
* Poor health	-0.001	0.49	-2.44	0.59
* Chronic conditions	-0.115**	-0.33**	-0.12	-0.108*
* Health limits social life	0.04.	0.32	0.68	-0.07
* ADLs	0.103	0.57	0.12	0.18
* IADLs	-0.005	-0.03	-0.04	-0.20

* = statistically significant at the 5% level; ** = statistically significant at the 1% level

Table 7.11. Summary: Effect of HMO Membership on Utilization

Dependent Variable	Big Panel Naïve xtprobit	Big Panel IV xtprobit	Big Panel DFM logit[1]	Small Panel Naïve xtprobit	Small Panel IV xtprobit	Cross-sect'l Naïve probit	Cross-sect'l IV probit
Pr(Hospitalization)	-0.511**	-0.460**	-0.716**	-0.411**	-0.67**	-0.404**	-1.24**
# of Inpatient Days	-0.212**	-0.01	-0.435**	-0.18	-0.01	-0.308**	-0.41*
Pr(Physician Visit)	0.245**	0.125	1.481**	0.046	-0.15	0.289**	0.10

* = statistically significant at the 5% level; ** = statistically significant at the 1% level

[1] The DFM estimates logit coefficients, but the coefficients presented in this Table have been scaled to represent probit coefficients, for ease of comparison.

While several possible explanations exist for differences in significance levels, there is no easy way to account for these differences in sign. Since the deviations are not specific to one dataset, there seems to be little basis for concluding that the effects vary because the samples are different (e.g., because one sample includes persons who died). Clearly one of the signs is "wrong" in the sense that it misstates the direction of the "true" population effect, but it is hard to say why it is wrong.

Explanations of the differences in the significance levels of the variables are of two basic types: the sample composition explanation and the estimation method explanation. The sample composition explanation is that the differences in the samples of the Medicare population captured by each of the three datasets are sufficiently large that they influence the observed effects. The most obvious difference between the datasets is their sample size. The big panel dataset has 38,185 observations; the cross-sectional, 13,373; and the small panel, just 7,432. Of these, 4,642 in the big panel are HMO members; 2,958 in the cross-sectional dataset are HMO members; and 696 in the small panel belong to HMOs. Moreover, the big panel dataset includes 7,338 persons who had at least one hospitalization, while the cross-sectional dataset has only 2,171 such persons, and the small panel only 1,169.

The size of a sample exerts an powerful impact on the power of a regression analysis to detect an effect. The results of a power analysis conducted on a cross-sectional formulation of the inpatient days model prior to undertaking this study gives some indication of the magnitude of the impact. The results are not directly applicable to the final inpatient days model used in this study because the model specification changed somewhat (i.e., variables from the Area Resource File were added to it), but it gives a rough indication. The analysis followed the methodology for power calculations in multiple regression described by Cohen and Cohen (1983). The calculations determined that a dataset containing 7,338 hospitalized persons would have 90 percent power to detect a difference of 3 inpatient days. In contrast, a dataset with only 2,171 hospitalized persons would only be able to detect a difference of 5 days at 90 percent power; it could detect a difference of 4 days at 75 percent power; and its power level for a 3-day difference would be below 70 percent. A dataset with only 1,169 hospitalized persons could detect a 5-day difference at 70 percent power, but its power to detect a 3- or 4-day difference would be under 70 percent. These are quite significant differences, and they may explain why the HMO variable was found to have a significant effect in the big panel DFM inpatient days estimation and the cross-sectional estimation, but not in the small panel estimation. (Note that this explanation does not, however, account for the statistical insignificance of the HMO variable in the big panel IV estimation. We must turn to other explanations for that disparity, such as the tendency of IV estimation to increase standard errors.)

There are two other important differences between the sample compositions of the datasets. One difference is, of course, that the two panel datasets include multiple

years of data, while the cross-sectional dataset consists entirely of 1996 data. Because an individual's health status in any given year may deviate substantially from her usual health status due to an unusual acute health event, such as a heart attack, the cross-sectional data may not provide as accurate a picture of sample members' health status than the panel datasets. By incorporating several years of health status data, the panel datasets enable more valid inferences to be drawn about the impact of health status on HMO choice and health services utilization. However, the results do not indicate that the relationship between health status and either of the dependent variables was dramatically different in the cross-sectional estimations than in the panel estimations. With few exceptions, the health status variables were similarly signed and achieved similar significance levels. Thus, this difference between the sample compositions does not seem to bear much responsibility for the observed differences in the results. Additionally, it should be remembered that panel and cross-sectional models illustrate two different effects: results from panel estimations indicate responses to change over time, while cross-sectional analyses show cross-sectional differences.

The datasets also differ as to whether or not they include persons who died. While no dataset includes persons who died for the year of their death, the big panel dataset does include persons who died for the years prior to their death. The small panel could not include these persons because longitudinal weights were not available for persons who were not in the sample for all 12 months of all 4 years studied. The cross-sectional dataset does not include deaths because it is drawn from the Access to Care File, which consists only of continuously enrolled persons. It is worth repeating that the big panel estimation of the physician visits equation did not include deaths,

because the physician visits variable is also drawn from the Access to Care File. The other equations, however, were run including persons who died. This may have made a difference in the results, especially as concerns utilization, because it is well known that people typically consume an enormous amount of health care services in the six months preceding their death.

While there were only a total of 911 deaths in the big panel dataset (2.39 percent of the total sample), this may have been enough to influence the results. Persons who died were much less likely to be HMO members: only 3.18 percent of the persons who died were HMO enrollees, as compared to 12.38 percent of those who did not die. A look at the crosstabs for utilization by survival status also reveals vast differences in consumption levels. The mean number of inpatient days (given hospitalization) for persons who died was 43 days, as compared to just 11 days for persons who survived. The probability of hospitalization was also much higher for persons who died: 59.82 percent versus 18.23 percent. Persons who died are clearly outliers on these dependent variables. Their presence in the big panel dataset, even though they constituted only a tiny fraction of the sample, may have influenced the results. This is not a bad thing; it does not constitute bias. It simply means that the big panel results represent the effects that are present in the Medicare population as a whole (including deaths), while the small panel and cross-sectional results are only generalizable to the continuously enrolled Medicare population.

The discussion of the differences in results between the datasets thus far has focused on sample composition explanations. It is probably also the case, however, that the results can be explained by differences in the estimation methods used. One

important factor is that the big panel estimations did not incorporate survey weights, while the small panel and cross-sectional regressions did. As was discussed in Section 4.4.6, failure to incorporate sampling weights into a complex survey data analysis can lead to bias in both the regression coefficients and their variances. The typical effect is to overstate the statistical significance of the coefficients.

An investigation determined that there would be no significant gains associated with adjusting the standard errors for the design effect for the models in this study. However, the magnitude of effect of omitting sampling weights from the big panel analysis is unknown. It is possible that that big panel regression coefficients are biased because of this omission. This might explain some of the differences in the results between datasets. There are two mitigating factors, however. First, researchers have noted that longitudinal analyses that follow individuals over time are relatively free from the concerns stemming from failure to take into account complex survey design. They note that "[t]he little work that has been done has shown that conclusions from such analyses are the same whether the complex design is taken into account or not" (National Center for Health Statistics, 1992). Second, in theory such adjustments are not needed where the model is fully specified. It seems that the utilization models, at least, are close to fully specified, since the R^2 they achieved is at, near, or above (depending on the particular equation and dataset in question) the maximum achievable R^2 for health services utilization models as determined by prior studies. Thus, the omission of survey weights from the big panel analysis may not be as big a problem as might be imagined.

Another methodological explanation for the differences in results across the different estimations in this study relates to the choice between full information maximum likelihood (FIML) and limited information maximum likelihood (LIML) techniques. The discrete factor model is a FIML model, while the two-stage instrumental variables model uses LIML estimation. LIML estimation incorporates "limited information" in that it estimates parameters based only a single equation. FIML estimation, in contrast, incorporates the "full information" from the entire system of equations.

LIML methods have several advantages over FIML. In additional to their computational ease, they are able to produce unbiased coefficients (when there are no lagged endogenous variables) which will not be affected by specification errors in other equations. In contrast, FIML parameter estimations may be biased for all equations if there is specification error in any of the equations. A major disadvantage of LIML methods, however, is that they involve a loss of precision. The LIML estimator is efficient among single-equation estimators, but with normally distributed errors, the FIML estimator is efficient among all estimators for simultaneous equations (Greene, 1993). Thus, standard errors will be larger, and significance levels lower, under LIML than under FIML.

This may account for one of the most important discrepancies in the results between datasets: the finding that for both the inpatient days model and the physician visits model, HMO membership achieved statistical significance in the discrete factor estimation but not in any of the instrumental variables estimations (with the single exception that it was statistically significant in the IV estimation of the inpatient days

equation on the cross-sectional dataset). Sample size may explain the differences between the DFM results and the results for the small panel and cross-sectional datasets, but the loss of precision associated with LIML as compared to FIML appears to be the soundest explanation for the differences in the DFM and IV results for the big panel dataset.

The DFM estimates may also be more precise than the IV estimates because the DFM in a semiparametric model whose results are robust to changes in the assumptions about the error distribution. Two-stage least squares estimates, in contrast, are based on the assumption of normally distributed disturbances. When this assumption proves incorrect, the least squares estimator is still best linear unbiased, but it is not best unbiased. That is, there exist nonlinear estimators that are more efficient. The DFM estimator is one of these.

In fact, it appears that the errors in the two-stage least squares model were not normally distributed. The SKTEST command in STATA was used to test the normality of the disturbance in the 2SLS estimation. For the big panel dataset, the null hypothesis that the distribution was normal was rejected at the 1 percent level for the skewness test, the kurtosis test, and the joint skewness-kurtosis test (for the joint test, ($\chi^2(2)=11.35$, $p=0.0034$). The same results were found for the cross-sectional dataset (for the joint test, ($\chi^2(2)=10.93$, $p=0.0042$). For the small panel, however, the normality hypothesis was not rejected. The p-value for the skewness test was 0.0643; for the kurtosis test, $p=0.823$; and for the joint test, $\chi^2(2)=0.27$, $p=0.8759$. Thus, it appears that the assumption of normally distributed errors may be true for the cross-

sectional 2SLS estimation, but not for the big or small panel estimation. The implication is that the 2SLS estimates are less efficient than the DFM estimates for the panel estimations. As was discussed in relations to the FIML versus LIML issue, this loss of precision may explain why the HMO variable was significant in the DFM regression but not in the 2SLS regression.

The efficiency problem for the 2SLS estimation may be compounded by the fact that it was not possible to perform the manual 2SLS correction to the second-stage standard errors. This correction accounts for the fact that predicted rather than actual values of the endogenous variable are used as instruments in the second stage regression. The caveat that failing to perform this correction may compound the efficiency problem in the 2SLS estimation arises from the lack of certainty about the direction of the bias. Gujarati (1995) states that the uncorrected standard errors will not be the "true" standard errors, but does not indicate whether they will be biased upward or downward. The 2SLS stan\dard error adjustment was not possible for the models in this study because XTGEE does not report the sum of squared errors, a necessary component of the correction factor. The discrete factor model, of course, does not have this problem because it does not use 2SLS. It also is not a problem for the two-stage probit models, because of the research indicating that there are no significant gains associated with making the standard error adjustment in those models. It might, however, explain some of the disparity in the significance of the HMO variable in the inpatient days model under different estimation methods.

Given all of the above considerations—sample size, inclusion of persons who died, panel versus cross-sectional data, sampling weights, FIML versus LIML,

assumptions about the error distribution, and standard error adjustments—which set or sets of results should one rely upon as the most accurate reflection of the true population parameters? There is no obvious answer, as each of the models has drawbacks. The DFM seems to be the clear winner on most criteria—particularly power, efficiency, robustness, and generalizability to the entire Medicare population—but it must not be forgotten that this model does not include sampling weights. The DFM results should certainly be preferred to the big panel IV results, however, given the trouble with the normality assumption. The small panel and cross-sectional models both include sampling weights, but the nonnormality of the errors casts doubt upon the precision of these estimates. Moreover, the statistical power of the small panel inpatient days model is not high, and results based on the small panel and cross-sectional datasets can only be generalized to the continuously enrolled Medicare population. Since the utilization patterns of persons approaching death is of great policy interest to those seeking to control Medicare costs, this is regrettable. Overall, the choice of a favorite model depends on one's assessment of whether the DFM's benefits in terms of power and precision outweigh the possible bias introduced by the lack of sampling weights. If one credits the suggestion that sampling weights are not necessary in a fully specified model and/or a model that tracks a panel of individuals over time, then the DFM would be the preferred model.

8. Discussion

8.1. Market penetration and favorable selection

8.1.1. Recap of DFM results

In the discrete factor estimation, one of the five general health status variables (excellent health) was statistically significant, and two of the three chronic conditions variables were significant. The magnitude of the effects, however, was fairly small. For instance, having excellent health increases the average probability of HMO membership by 0.61 percentage points (11.42%). Thus, the discrete factor results provided evidence of only a very weak favorable selection effect.

The results also failed to indicate a significant effect of Medicare HMO market penetration on the degree of favorable selection into HMOs in a particular county. Of the six interactions of market penetration with health status measures, only one was statistically significant.

8.1.2. Theoretical discussion

This study investigated whether there is evidence of continued favorable selection in the 1990s, and whether the extent of favorable selection in a county varies with the degree of Medicare HMO market penetration. It was hypothesized both that the extent of favorable selection in a particular county would vary with the county's HMO market penetration level, and that the overall level of favorable selection in Medicare HMOs would be very low. Four theoretical reasons were presented for these

hypothesized effects: (1) HMOs in high-penetration markets are more likely than HMOs in low-penetration markets to recruit Medicare beneficiaries who health is relatively poor; (2) beneficiaries in poor health will be more familiar with the concept of managed care, and thus less hesitant to join an HMO, in mature markets; (3) rates of physician contracting with HMOs are higher in mature markets, making it less likely that a chronically ill Medicare beneficiary in those markets would have to break an existing physician tie in order to join an HMO; and (4) the enrollee population of Medicare HMOs in mature markets will have aged since the early days of the risk program, with attendant consequences for its illness risk profile.

The results of this study indicate that the overall level of favorable selection in Medicare HMOs in the 1992-96 period *is* quite low. However, there is no real evidence that the extent of favorable selection in a particular county is significantly affected by the county's HMO market penetration level.

In order to determine the strength of the favorable selection dynamic in Medicare HMOs overall, an individual's choice to join or not join a Medicare HMO was regressed on several health status variables and sociodemographic variables that are correlated with health status. Under the theory of health plan choice presented in Section 3.1, continued favorable selection would be indicated by significant positive coefficients on those variables that indicate poor health and negative coefficients on those that indicate relatively good health. Additionally, the under the theory of plan choice, there should be a negative and statistically significant relationship between

income and HMO membership, since those with low incomes gain the most utility from the lower out-of-pocket costs associated with HMOs.

The results show that while the predicted relationships of income and other sociodemographic variables to HMO choice were present, there was little evidence of favorable selection on most of the health status variables. Five different lagged general health status indicators were included in the HMO choice model: excellent self-perceived health, poor self-perceived health, number of ADLs, number of IADLs, and a dummy variable representing whether the person's health limited their social life. While the signs on these variables were generally consistent with a favorable selection effect, only one variable—excellent self-perceived health—was statistically significant. Moreover, the effect on the probability of HMO membership for this variable was quite small, on the order of a 0.6 to 2 percentage point (11-16%) increase. In addition to these general health status measures, three variables indicating whether or not the individual had a history of arthritis, cancer, or stroke were included in the model. The significance levels of these variables varied across the different estimations; for the DFM, arthritis and stroke were significant, while cancer was not. Only stroke had a negative coefficient, consistent with a favorable selection effect.

In summary, there is only very weak evidence of favorable selection. Only 2 of 8 health status variables were statistically significant, and the magnitude of these effects was small. The conclusion to be drawn is that the overall degree of favorable selection into Medicare HMOs was quite mild during the 1992-96 period. Direct comparison of this finding to the results of studies from the 1980s is difficult because of the different

study methodologies and selection bias measures used. However, one thing that can be said is that this study found no evidence of continued favorable selection on many of the health status measures tested in earlier studies. The Mathematica/MCV study of Competition Demonstration HMOs, for example, found statistically significant favorable selection on a range of measures such as self-perceived health, functional disability measures, and bed days (Brown, 1988). Similarly, Retchin and colleagues (1992) found that there were significant differences between HMO enrollees and nonenrollees in the 1978-85 period on the presence of major health problems, the number of bed days and restricted activity days, ADLs, and IADLs. Other studies also found evidence of favorable selection on functional status measures (Lichtenstein et al., 1991; Hill and Brown, 1992; Riley et al., 1996). The present study identified no such effects. Rather, only the variable for excellent self-perceived health was significant in this study.

While the study proved that favorable selection in the 1992-96 period was not strong, it did not provide support for the hypothesis that the degree of favorable selection in a given county is a function of HMO market penetration in that county. To test this aspect of the hypothesis, six interactions terms interacting percent penetration with various health status measures were included in the HMO choice model. The coefficients on these interaction terms indicate the extent to which the relationship between health status and the probability of joining an HMO—that is, the favorable selection effect—is affected by the degree of HMO market penetration in a county.

The regression results provided little evidence of an interactive effect between market penetration and favorable selection. Of the six interaction terms, only one—the interaction of market penetration and the number of chronic conditions—is statistically significant. The coefficient is negative, indicating that high market penetration weakens the negative relationship between having chronic conditions and choosing an HMO. In other words, it weakens the favorable selection effect, as hypothesized. But again, this effect is only observed for one out of the six interaction terms. Given these results, it is somewhat surprising that a Wald test of the six interaction terms finds them to be jointly significant for the big panel dataset ($\chi^2(6) = 15.21$, $p=0.0187$). However, they were not jointly significant in the cross-sectional dataset ($\chi^2(6) = 8.70$, $p=0.1911$) or the small panel dataset ($\chi^2(6) = 5.84$, $p=0.4411$). Viewing all of the results together, the models do not provide significant support for the theory that market penetration significantly affects the favorable selection dynamic.

One possible explanation for the lack of significance of the interaction terms is that the variables were defined in a way that obscured real world effects. For example, the fact that market penetration was modeled as a continuous variable may have resulted in a failure to detect effects that are only visible above or below a certain market penetration threshold. The same might be postulated about modeling ADLs and IADLs continuously. A second possible explanation for the lack of significant coefficients is that so many different interaction terms were included, all capturing different dimensions of health status, that a problem of multicollinearity resulted. This

would bias standard errors upward and push the significance levels of the interaction terms down.

To test these hypotheses, the HMO choice equation was rerun multiple times using a number of different variations in specification. A total of 14 different regressions were run, incorporating the following experiments:

- recoding the ADLs and IADLs variables (both interaction terms and main effects) to single binary variables indicating the ability to perform 3 or more ADLs or IADLs;
- recoding the continuous market penetration variable to two dummy variables, low penetration (less than 5 percent penetration) and high penetration (more than 20 percent penetration), with the middle category omitted; and
- running the model with fewer interaction terms. The interactions of market penetration with "health limits social life" and excellent health were dropped. Different subsets of one or more of the remaining interactions (poor health, ADLs, IADLs, and chronic conditions) were tested. Moreover, each combination was tested with both the continuous and the dummy variable specification of the market penetration variable.

Each of these experiments was run on two different datasets, the big panel (XTPROBIT estimation) and the cross-sectional dataset. The results were remarkably uniform: the coefficients on the interaction terms remained consistently insignificant. Out of the 28 models, only three had even one statistically significant interaction term (continuous market penetration * history of stroke). All of these were big panel models. Wald tests revealed only these three models to have jointly significant sets of

interaction terms. The other 25 models all had no statistically significant interaction term coefficients and Wald tests that failed to reject the null. The robustness of these results to changes in the interaction terms and sample composition suggests that the finding of no statistically significant interactive effects cannot simply be attributed to poor variable definition or model specification choices.

The fact that this study did not find a strong relationship between favorable selection and market penetration in a particular county, but did find that favorable selection has diminished overall, is somewhat curious. If market penetration does not affect favorable selection, why has there been a large reduction in the amount of favorable selection into HMOs in the last decade or so? Is the fact that nationwide Medicare HMO market penetration has increased substantially over this same period mere coincidence?

One way to reconcile these results is to postulate that market penetration *does* affect favorable selection, but that its effects are not county-specific. It was hypothesized that four different effects drive the relationship between market penetration and favorable selection: HMO recruiting patterns, physician contracting patterns, consumer familiarity with managed care, and the aging of the HMO enrolled population. It may be that all of these factors are affected by managed care penetration, but that not all of them are affected by penetration on the county level. HMO recruiting patterns and the aging effect seem necessarily specific to a particular market area. But it seems plausible that consumer familiarity with managed care might be more a function of the increasing *national* penetration of managed care organizations

into the Medicare market and the publicity given the Medicare risk program nationwide by the media and HCFA's informational campaigns. That is, consumers all over the country may have become more familiar with managed care as a concept over the last ten to fifteen years, irrespective of whether the particular county in which they reside has high managed care penetration. Thus, it may be that individuals in relatively poor health *throughout* the United States have become more disposed to join an HMO. This would help account for a finding of an overall decrease in favorable selection but a lack of variation in favorable selection between counties according to the degree of local market penetration.

Similarly, the theory that physician contracting rates affect a chronically ill individual's likelihood of joining an HMO, and that physician contracting rates vary with managed care penetration, may be correct generally but not correct in its focus on inter-county variations. While there does seem to be some indication that contracting rates vary regionally in tandem with managed care penetration (see Table 3.3), this association may not be observable on a county level. Alternatively, it could be that the relationship between physician contracting and a chronically ill individual's likelihood of joining an HMO is not linear, but rather flattens out after a certain level of contracting has been attained in a locality, or does not begin to rise until after a threshold level has been attained. This would make the effect difficult to detect where market penetration is modeled as a continuous variable, or as dummy variables with cutoff points at levels other than those of the true threshold.

Thus, one explanation for this study's failure to identify a significant relationship between market penetration and favorable selection lies with the study's use of counties as the unit of analysis. A second explanation is that there may be insufficient variation in the market penetration data in this study to detect an effect. While the range of the market penetration variable was quite wide, ranging from 0.0056 percent to 51.56 percent, most of the counties were concentrated in the left tail of the distribution. The median penetration level was 1.88 percent; the 25th percentile was 0.067 percent and the 75th percentile was 12.79 percent. Only slightly more than 10 percent of the big panel sample lived in counties with more than 30 percent penetration, and less than 5 percent lived in counties with more than 40 percent penetration. It is possible that the relationship between market penetration and favorable selection is nonlinear, and visible only above a certain threshold. If this threshold is fairly high—say, 40 or 50 percent—there may have been too few persons in the sample residing in counties with that level of penetration to be able to detect the effect.

For all of the above reasons, the fact that this study failed to find a statistically significant association between Medicare market penetration and favorable selection does not necessarily mean that the theory offered on this point is incorrect. The results may well be attributable to study methodology and/or data constraints. The possibility of a high threshold effect seems particularly plausible. But on their face, the study findings suggest that the theory concerning market penetration was not correct.

Favorable selection has diminished over time, but county-level variations in favorable selection cannot be attributed to differences in the level of HMO market penetration.

8.1.3. Policy implications

This study's finding that there existed only weak favorable selection into Medicare HMOs in the 1992-96 period is of considerable significance to policymaking concerning the reimbursement mechanism for Medicare risk contractors. Since the early 1980s, when studies of demonstration HMOs first uncovered evidence of strong favorable selection and overpayment of risk contractors, there has been a flurry of research activity by analysts at HCFA and academics aimed at improving the payment formula. These efforts have yielded a large body of literature on Medicare risk adjustment. A number of proposals have been put forth for revamping the AAPCC formula. Most attempt to include additional individual-level illness risk adjusters in the formula; the two leading methods at the moment are diagnostic cost groups (DCGs) and ambulatory diagnostic groups (ADGs) (Newhouse et al. 1997). Some proposals also take aim at the dramatic and often inexplicable geographic variations in the AAPCC (Luft 1998). Virtually all of the researchers in this area are united in their belief that the existing reimbursement formula is "crude" and inadequate (Newhouse et al. 1997).

But this study suggests that this "crude" formula may not be cause for great concern after all. While it may be true that the risk factors included in the AAPCC formula (age, sex, disability status, institutional status, employment status, and Medicaid eligibility) collectively explain less than one percent of the variation in health care expenditures among Medicare fee-for-service beneficiaries (Dowd et al. 1992), it

does not appear to be the case that HCFA is grossly overpaying its risk contractors. Biased selection is only a policy problem—that is, it only results in incorrect reimbursement—if *both* of two conditions are met: (1) HMO enrollees are not representative of the Medicare population on some characteristics that are associated with health services utilization, and (2) those differences are not taken into account by the payment formula. Condition (2) does appear to be present, because only a small number of risk factors are included in the payment formula and these factors collectively have very low predictive power vis-à-vis health services utilization. However, this study suggests that condition (1) may not be satisfied. The study indicates that Medicare HMO enrollees are not significantly healthier than nonenrollees on most measures, and that overall the level of favorable selection is quite mild. Therefore, biased selection may not present as significant a policy problem as it did in the early years of the Medicare risk program.

This study's findings regarding favorable selection are also significant in that they resolve a major paradox in the Medicare risk program: If Medicare HMOs are being overpaid, why are they choosing to exit the Medicare market in droves? The autumn of 1998 saw the exodus of 43 risk contractors from the Medicare risk program, and reductions in service areas by an additional 52 HMOs, following the imposition of a 2 percent rate increase cap for Medicare HMOs as part of the 1997 Balanced Budget Act (Green 1998). Some 440,000 Medicare beneficiaries in 22 states (6 percent of the total enrolled population) have been affected by the pullout (Cardin 1999), prompting

media accounts of beneficiaries "in a panic" after being "sacked" by their HMOs (Russell 1998).

The managed care companies explain their market exit as a simple matter of economics: Medicare reimbursement is not adequate to cover the cost of care for their enrollees (Manning 1999; Aston 1998). Major managed care companies such as United HealthCare Corp. claim record losses in 1998 due to their participation in the Medicare risk program (*Modern Healthcare* 1998). It is unclear whether they have actually sustained losses, or merely have failed to realize the profit margins they had expected on their Medicare product line. But their complaints are certainly difficult to reconcile with HCFA's view, as stated in an October 2, 1998 House Commerce Subcommittee hearing, that "There is considerable evidence that we have overpaid plans and continue to overpay plans. These widely publicized withdrawals can be misleading, though." (Aston 1998). Another HCFA official questioned, "If plans felt they could make it work before, then why can't they make it work now?" (Jacob 1998).

The reason is that times have changed. HCFA apparently did overpay risk plans in the 1980s, but may not be doing so in a significant way today. The reason is, again, that the favorable selection which gave rise to this overpayment problem has dissipated over time. This explains why HMOs were able to make a profit from serving the Medicare market in the 1980s, but find themselves unable to stay in business today.

8.2. The effect of HMOs on health services utilization

8.2.1. Recap of DFM results

The discrete factor estimation determined that HMO membership has a negative and statistically significant effect on the probability of hospitalization. HMO membership decreases the average probability of hospitalization from 18.76 percent to 7.58 percent, a drop of 59.59 percent. In the DFM estimation (unlike in the instrumental variables estimation), HMO membership was also found to significantly decrease the number of inpatient days for those hospitalized (β=-0.696 in this log-linear model). Finally, and again in contrast to the instrumental variables estimation, the discrete factor model identified a positive and statistically significant correlation between HMO membership and the probability of seeing a physician. The average probability of seeing a physician increases from 74.89 percent to 88.99 percent (a 14.10 percent change) when the HMO membership is varied for all observations.

This study's findings concerning the effect of HMOs on health services utilization reinforce the conclusion that there is no further room for reducing reimbursement to Medicare risk contractors. Before proceeding to a discussion of this policy implication, however, a discussion of the theoretical and research implications of the study's findings concerning HMOs and utilization is in order.

8.2.2. Modeling the relationship

This study provides insights for future researchers who seek to model the relationship between HMO membership and subsequent health services utilization. It

answers two important questions: First, is it *necessary* to model the relationship using simultaneous equations? Second, is it *possible* to model the relationship with simultaneous equations?

The necessity of using simultaneous equations turns on the question of whether HMO membership is an endogenous variable. The concern with endogeneity is that there are some *unobserved* variables that affect both HMO choice and subsequent utilization. These variables are represented in the error terms for the HMO choice and utilization equations. Consequently, the error terms of the two equations are correlated. This causes OLS estimates to be biased and inconsistent, since the endogenous HMO variable in the utilization equation is correlated with the error term, violating the assumption that $cov(X_i, u_i)=0$.

In this study, the Pindyck and Rubinfeld test produced the unexpected result that the HMO variable was not endogenous. This result was invariant across datasets. How should we interpret this result, given that all research to date has indicated that there are variables—in particular, health status indicators—that affect both HMO choice and utilization? There are two possible explanations. It may be that in fact, health status does not affect both variables. The regression results suggested that for these data, there was only a weak favorable selection effect. On most health status measures, there was not a statistically significant association with health plan choice. Thus, HMO choice may be exogenous with respect to health care utilization. A second, and more likely, possibility is that there are variables that affect both HMO choice and utilization, but that the models in this study successfully captured all of

them. Thus, there was no endogeneity because there were no *unobserved* variables affecting both HMO choice and utilization. Even if one believes that favorable selection has diminished, so that health status variables no longer affect both dependent variables, there may still be other variables that bear on both. Indeed, the following non-health status variables were statistically significant in both the HMO equation and the one or more of the utilization equations: age, education level, Medigap coverage, marital status, nursing home residence, usual source of care, income, Medicaid coverage, rural/urban residence, and county AAPCC rate. One might postulate, then, that the finding of no endogeneity in this study is attributable to *both* of the above explanations. The diminution of favorable selection has meant that unobserved health status did not end up in the error terms of both equations, and the inclusion of a wide range of sociodemographic variables that affect both HMO choice and utilization has meant that these effects did not end up as part of the errors.

What are the implications for future research? The evidence suggests that a fully specified utilization model may be estimable through OLS or other single-equation methods. So long as all of the variables found in this study to significantly affect both HMO choice and utilization are included in the utilization equation (along with the righthand side HMO variable), parameter estimates from single-equation methods should be unbiased. Leave one of these variables out, however, and it is necessary to revert to simultaneous equations methods.

A final caveat about endogeneity: Notwithstanding the above discussion and the Pindyck and Rubinfeld results, there is some reason to suspect that the hypothesis of

endogeneity cannot be completely ruled out in this study. If HMO membership was truly exogenous, then OLS estimates for the utilization equations would be unbiased, and we would expect to see no significant differences between the results of a naïve estimation and a simultaneous equations estimation of the utilization models. The results of this study are generally consistent with this proposition—for most of the utilization regressions, there were no appreciable differences in the magnitude and significance of the effects of the righthand side variables on the utilization measures between the naïve and the simultaneous equations estimations. However, there were exceptions. Compared to the simultaneous equations results, the naïve model overstated the significance levels for many variables in the inpatient days and physician visits equations for the big panel dataset, and in the physician visits equation only for the cross-sectional dataset. Thus, there may be a small amount of endogeneity not detected by the Pindyck and Rubinfeld test.

The foregoing discussion has argued that the effect of HMO membership on utilization may be modeled using single-equation methods if the dataset is rich enough that the utilization model is fully specified. However, one must use simultaneous equations if some of the variables known to affect both HMO choice and utilization cannot be represented in the model. Future researchers might encounter this problem if their datasets did not contain information on key health status indicators. Given that simultaneous equations would be necessary in this situation, one might well ask: has this study demonstrated that it is possible to successfully model the relationship using this method? The answer is a qualified yes. The key issue, of course, is identification.

Simultaneous equations have not previously been used in studies of the impact of Medicare HMOs on utilization because researchers have been unable to find variables to use as exclusion restrictions to identify the system. This study used market penetration indicators for this purpose. By and large, the experiment seems to have been successful. The Norton and Staiger test for all three datasets determined that the market penetration variables were good instruments. The exclusion restrictions made a significant joint contribution to the HMO choice equation. The results for the Norton test were more mixed, but generally supported the proposition that the variables were good instruments. The restrictions were found not to significantly impact utilization in 6 of the 9 Norton tests run (one test for each of 3 equations for each of 3 datasets). They were, however, found to be jointly significant in the inpatient days equation for the big panel and cross-sectional datasets, and in the physician visits equation for the small panel dataset only. The differences in test results across datasets are somewhat puzzling. The failure of the Norton test results to lend unmitigated support to the identification strategy pursued in this study should be taken into consideration by future researchers. Overall, however, the strategy seems to have been a sound one. Future research might investigate whether market penetration would make a better exclusion restriction if it was modeled as a series of dummy variables rather than a continuous variable.

8.2.3. Theoretical discussion

This study hypothesized that Medicare HMO enrollees would have slightly lower health services utilization than nonenrollees when sociodemographic and health

status differences were controlled for. While studies from the 1980s found significant effects of Medicare HMOs on utilization, those studies were conducted in an era in which favorable selection into HMOs was clearly present, and the study methodologies did not contain good controls for selection bias. Thus, some of the effects attributed to the HMO utilization control mechanisms may actually have been due to the fact that the baseline health of the Medicare HMO enrollee population was better than the that of the fee-for-service population. The present study utilizes data from an era in which favorable selection is less of a concern *and* includes strong controls for any persistent selection bias that might exist; thus, it was expected that the residual effect of HMOs on utilization identified by the study would be rather small.

Contrary to this hypothesis, the study found a significant effect of Medicare HMOs on the probability that a Medicare beneficiary will be hospitalized in a given year. The effect of HMOs on the predicted number of inpatient days used by those persons hospitalized was large in magnitude, and statistically significant in the DFM estimation, but failed to achieve statistical significance in some of the other estimations. With respect to the probability of hospitalization, the HMO membership variable was negatively signed and statistically significant at the 1% level in all four estimations performed (big panel IV, big panel DFM, small panel IV, and cross-sectional IV). HMO membership altered the average probability of hospitalization by 10 percentage points, which represented a 46-63% change in the panel estimations and 20% change in the cross-sectional estimation. To put this result into perspective, Langwell and Hadley's 1989 study of the early years of the Medicare risk program identified a 14 to

25 percent reduction in hospital use (due to fewer admissions) due to Medicare HMOs; the RAND study of working-aged persons in HMOs found a 40 percent decrease in hospital admissions (Manning et al., 1984); and Brown and colleagues (1993) found no appreciable effect on hospital admissions.

The present study found a sizeable effect of HMOs on the number of hospital days used by those hospitalized. The HMO variable was statistically significant in two of the estimations (DFM and cross-sectional), but not significant in the other two (big panel IV and small panel). The magnitude of the effects found, however, was fairly large in all of the estimations, on the order of a 5 day (43%) decrease. Previous studies of Medicare HMOs found that HMO membership was associated with a 16.8 percent decrease in length of stay (Brown et al., 1993). Studies of non-Medicare HMOs typically find reductions in hospital days on the order to 10 to 40 percent (Manning et al., 1984; Stern et al., 1989; Luft, 1981). The HMO effect achieved statistical significance in most, but not all, prior studies. The results of the present study concerning inpatient days, while variable across datasets, generally indicate a comparable or larger HMO effect than that identified in other studies.

This study determined that HMO membership does not have a substantial effect on a Medicare beneficiary's likelihood of seeing a physician on an outpatient basis at least once during the year. It appears to nudge this probability up slightly: the increase was in the neighborhood of 2 percentage points (2-3%) for all of the estimations except the discrete factor model, in which it was 14 percentage points (18.83%). However, the change only achieved statistical significance in the discrete factor model. These

findings are consistent with the findings of earlier studies. Luft's (1981) review of 26 studies of non-Medicare HMOs concluded that most failed to find a significant effect on physician visits. Brown and colleagues (1993) found that Medicare HMO members had a 5 percent higher probability of seeing a physician at least once (but a slightly lower probability of having one or more physician visits per month). Thus, the hypothesis that the effects of HMOs on utilization would be lower in this study than in previous studies was not proven correct for physician utilization. This may be because HMOs truly do not affect physician utilization, or it may be because their effect on physician utilization is limited to increasing or decreasing the *total number* of physician visits in a year given some utilization. This study originally set out to model the total number of physician visits, but was unable to do so because of data constraints (namely, the unreliability of MCBS data on total number of physician visits for HMO members, who do not have claims documentation of their usage). Future research might be aimed at examining this relationship. This study is only able to conclude that HMOs do not have a significant effect on the probability of having at least one physician visit. It is quite possible that HMOs might increase the likelihood of seeing a physician but decrease the total number of physicians in a given year, leading to a net reduction in physician utilization.

As a postscript to this discussion of the study's results concerning utilization, it is worth noting that the results provide strong support for the behavioral model of health services utilization put forth by Andersen and colleagues and adopted as the theoretical framework for this study. The behavioral model theorizes that health care

utilization is a function of predisposing, enabling, and need characteristics. The model predicts that characteristics such as advanced age, male sex, white race, and high educational attainment predispose individuals to consume health services; that high income, urban residence, and a plentiful local supply of physicians and hospital beds enable them to consume; and that poor health creates a need for consumption. The empirical results of the present study show the following relationships:

- *Age*: positive and statistically significant for the probability of hospitalization, but insignificant for inpatient days and the probability of a physician visit;
- *Male*: positive and statistically significant for both inpatient variables; negative and statistically significant for physician use;
- *College education*: positive and statistically significant for the probability of hospitalization and physician use; not significant for inpatient days;
- *Low income*: negative and statistically significant for inpatient days and physician use; not significant for the probability of hospitalization;
- *Medigap coverage*: positive and statistically significant for all models;
- *Census region*: some regions statistically significant for physician use, but for hospital use there is no strong effect;
- *Rural residence*: most categories not statistically significant, but some rural categories had negative and significant coefficients for each of the models;
- *Supply of physicians and hospital beds*: not statistically significant for any of the measures of utilization; and

- *Poor health*: most indicators of poor health were positive and statistically significant for all measures of utilization.

Thus, while few of the local area characteristics were found to exert significant enabling effects, the individual-level enabling characteristics, as well as most of the predisposing and need variables, displayed effects consistent with those hypothesized by the behavioral model. The behavioral model is very widely accepted and supported by a large body of empirical research, so this result is not particularly surprising.

8.2.4. Policy implications

This study found that when selection bias is controlled for, Medicare HMOs significantly decrease the probability of hospitalization, but effect only modest reductions in the amount of inpatient utilization by those who are hospitalized, and may actually slightly increase the likelihood of having some physician utilization. Is the reduction in hospital admissions sufficient justification for continuing the Medicare risk program? And what implications do these findings have for the AAPCC payment formula?

It is unclear from this study how this reduction in hospital *use* translates into a reduction in *costs*. This study did not analyze the specific nature of the hospitalizations that were avoided, or the types of inpatient stays that were shortened, by HMOs. Hospitalization episodes vary significantly in the type and intensity of care provided, and thus in the costs incurred. For example, a ten-day stay consisting of major surgery followed by a stint in the intensive care unit will be exponentially more costly than a ten-day stay on the general wards for low-intensity monitoring of condition such as

pneumonia. Thus, it is difficult to extrapolate from hospital days avoided to costs avoided without additional information about the nature of the medical condition and treatment modalities.

As a general matter, there is empirical evidence that the lower rates of hospital admissions among HMO members are not confined to discretionary hospitalizations, but rather cut across all hospitalization categories (Gillick, 1987; Wolinsky, 1981; Luft, 1981). This may be because HMOs are able to reduce hospitalizations in two different ways: they reduce the rate of hospitalization for relatively minor illnesses and procedures by substituting ambulatory for inpatient care, and they reduce the rate of hospitalization for more serious illnesses such as stroke, myocardial infarction, and cancer by doing a better job than fee-for-service plans of reducing the incidence of these conditions among their enrollees through preventive care. Thus, we might postulate that the hospitalizations that are avoided by substitution of outpatient care are of the relatively inexpensive type, but hospitalizations avoided through preventive care are of the more costly type. This study does not test this proposition, but it makes good theoretical sense. If it HMOs reduce hospitalization rates across, the board then we would expect to see the reduction in hospital use translate into a reduction in hospital costs of approximately equal magnitude.

One must be cautious in interpreting this finding in terms of cost savings *to the Medicare program*. Even if we assume that Medicare HMOs are able to achieve a significant reduction in hospital costs, this does not necessarily mean that Medicare is saving this much money. Medicare's savings depend on the level of reimbursement

given to HMOs by the AAPCC payment formula, and whether there is any lingering favorable selection that makes this formula inadequately risk-adjusted. The AAPCC is currently geared to save Medicare 5 percent of its total costs—that is, Medicare HMOs are paid 95 percent of what HCFA estimates it would cost to care for a Medicare beneficiary with that risk profile in the fee-for-service sector. If no favorable selection exists on a risk factor not represented in the payment formula, then the Medicare program will indeed save 5 percent. If Medicare HMOs are able to reduce hospital costs by more than 5 percent, the HMOs keep the difference (though they are required by law to devote it to reducing Medicare enrollees' premiums or expanding their benefits packages). That "extra" cost savings doesn't go to the Medicare program. Assuming no selection bias problem, Medicare will save exactly 5 percent due to the Medicare risk program as long as it maintains its present reimbursement formula.

Does this reimbursement formula, at its present level of 95 percent of thee average cost of caring for a fee-for-service individual, overpay HCFA's risk contractors? The Clinton Administration appears to believe that it does, as it has proposed to reduce the rate of reimbursement to 90 percent (Newhouse, Buntin and Chapman 1997). The 1995 Republican Medicare plan also proposed significant reductions in reimbursement (Oberlander 1997; Congressional Budget Office 1995; Physician Payment Review Commission 1995). Historically, when analysts have talked about the overpayment problem, they have been referring to overpayment arising from favorable selection into Medicare HMOs. That is, they have been speaking of the disjunct between a reimbursement formula that is based on the *average* cost of caring

for beneficiaries in the fee-for-service sector and the fact that, at least in the early years of the Medicare risk program, the beneficiaries who chose to join HMOs were much healthier than the average beneficiary. This study suggests that although there is some evidence of continued favorable selection, the effect appears to have become quite attenuated over time. Thus, the overpayment problem caused by favorable selection also may have become less salient.

There is a second sense in which HCFA might be overpaying its risk contractors, however. Even if one assumes completely neutral selection, a reimbursement rate based on 95 percent of the cost of average cost of caring for beneficiaries in the fee-for-service sector may be considered too high if HMOs are actually able to care for their Medicare enrollees at a dramatically lower cost than fee-for-service plans. If, hypothetically speaking, a risk contractor managed to provide for the needs of its Medicare members at 50 percent of the cost of a traditional plan (because, for example, it was able to obtain extremely favorable terms from the physicians with which it contracts), it would seem rather extravagant to reimburse it at 95 percent of the AAPCC for an extended period of time.

This study does not provide *direct* evidence for or against the proposition that HCFA might be overpaying Medicare HMOs in this sense, because this study did not use *costs* as its metric for measuring the effects of HMOs on utilization. The study's findings do suggest, however, that HMOs are able to significantly reduce the incidence of hospitalization among Medicare beneficiaries. The reduction is on the order of 10 percentage points (46-63% in the panel models; 20% in the cross-sectional model).

There is also evidence of a significant effect of HMO membership on the number of hospital days used by those hospitalized. The study results suggest that HMOs actually increase the likelihood of seeing a physician, the large effect on the probability of hospitalization raises a genuine issue as to whether HCFA might be compensating its contractors too generously. This possibility is certainly cast into doubt by the fact that risk contractors are choosing to exit the Medicare market at a high rate. However, it is a question that merits further investigation, in the form of studies that measure the effects of Medicare HMO membership on hospital *costs*, rather than on the probability of hospitalization and the number of inpatient days.

In conclusion, this is an era in which HCFA is engaging in broad experimentation with means of controlling costs in the Medicare program. Medicare managed care is but one of its experiments. The recent creation of the Medicare+Choice program is its most recent foray into alternative financing and delivery mechanisms. No doubt as the Choice program is implemented and evaluated, HCFA will reassess the relative attractiveness of the risk program. One aspect of this assessment is the efficacy of the program in saving money for the Medicare program. This study's findings are germane to that question in that they suggest that favorable selection may no longer be a significant problem, and thus that HCFA may actually now be saving close to the 5 percent it originally envisioned. The study's finding of a significant HMO effect on hospital use is also relevant to an evaluation of the risk program, for two reasons. First, it indicates that HMOs are effective in reducing utilization for the Medicare population, and thus that managed care is probably a

strategy worth preserving in the future, even if the means by which we reimburse managed care organizations require reform. Second, it suggests that there *may* be room for further reductions in reimbursement levels, if it can be shown that the reduction in the incidence of hospitalization corresponds to a real decrease in overall utilization costs. The fact that HMOs are able to achieve such a large reduction in hospitalizations yet are increasingly feeling financially strained is a paradox that merits substantial further investigation. One possible explanation for the paradox is that HMOs spend much more on ambulatory care and prescription drugs than fee-for-service plans, and that these expenditures offset the savings they achieve on the inpatient side. This study is not able to test this theory since it did not examine the effect of HMOs on the *level* of outpatient utilization. But given the finding that HMO members are more likely to see a physician, and the fact that HMOs offer generous prescription drug benefits to elderly enrollees, who are heavy users of prescription drugs, this explanation appears to be quite plausible.

The question of whether risk contracting saves the Medicare program money is, of course, not the only consideration that belongs in an assessment of whether managed care is a strategy HCFA should continue to pursue. The advantages that HMO membership can provide to Medicare beneficiaries are also relevant. While it is true that premiums are increasing, and benefits becoming less extensive, over time, most HMOs continue to offer a very attractive benefits package to elderly persons at a reasonable out-of-pocket cost. They enable Medicare beneficiaries to obtain access to Medicare Part B and other services at a cost that is typically considerably lower than

traditional Medigap premiums. Access to prescription drugs, vision and hearing services, and preventive care appears to be quite good in most Medicare HMOs, and these are crucial services for aging individuals. Moreover, to the extent that HMOs are able to keep elderly patients out of the hospital—both by preventing illness and by substituting outpatient care—they reduce patients' risk of having their medical conditions complicated by nosocomial infections and other health risks associated with hospital stays. Patients' overall quality of life may also be higher if their conditions are treated with ambulatory or home care where possible. Additionally, HMOs may provide case management of chronic conditions by coordinating care from multiple providers and facilitating integration of acute and long-term services in a way that is not common in fee-for-service plans (Oberlander 1997; Kane 1995; Spitz and Abramson 1987). Thus, even if it does not result in huge savings to Medicare or a lucrative business opportunity for HMOs, the Medicare risk program may be worth preserving for the sake of the beneficiaries who choose to take of advantage of it, and who are very satisfied with their choice.

9. Concluding comments: Study limitations and contributions

9.1. Limitations of the study

The methodology of this study has several limitations. The limitations are of two types: those arising from the data, and those arising from the estimation methods. Each of these will be discussed in turn.

First, the datasets used in this study have several shortcomings. The potential underreporting problem in MCBS data on health care utilization has already been discussed in Section 5.2.2. This study relied on inpatient utilization variables drawn from unreconciled claims data. An investigation was conducted into the degree of correspondence between unreconciled and reconciled data for the hospitalization variable, and the match rate was found to be extremely high. However, it was not possible to perform such a check for the number of inpatient days variable, because the MCBS does not publicly report interview data on the number of inpatient days. An inference was drawn that if the claims data were generally valid for the hospitalization variable, they probably were also valid for the number of hospital days, but this inference cannot be empirically proven with the available data. Thus, it is possible that there is an underreporting problem on the inpatient days variable for HMO members, because HMOs generally do not submit claims reflecting the number of days their members were in the hospital.

There is also a possibility of underreporting on the physician visits variable, because it is drawn from interview responses only; it is not reconciled with claims data. However, since the variable is merely binary, it seems relatively unlikely that there would be a great deal of inaccurate recall: most people can remember whether or not they went to the doctor at least once during the last four months. The possibility of underreporting cannot be ruled out, however.

This particular outpatient utilization variable was selected from among several other possibilities because it seemed to minimize the likelihood of underreporting. One unfortunate effect of this choice, however, was that persons who died had to be excluded from the outpatient model for the big panel dataset. The reason is that the physician visits variable comes from the Access to Care File only, and the Access to Care sample does not contain persons who died. Persons who died were included in the two inpatient utilization models, as the dependent variable for those equations was drawn from the Cost and Use File.

Combining 1996 Access to Care data with 1993-95 Cost and Use created other problems as well. For two of the righthand side variables, Medigap coverage and nursing home residence, the Access to Care File and the Cost and Use File contain different measures of the construct. Nursing home residence posed a particular problem since the 1996 file did not measure institutionalization in general, but only whether the interviews were conducted in a nursing home. Thus, it is possible that some 1996 observations were misclassified on the nursing home variable (e.g., if they happened to be interviewed in a community setting but spent most of the year in a

nursing home). Since interviews are conducted every four months, this seems like a fairly remote possibility, though.

Finally, there is the issue of sampling weights. The big panel dataset was conceived of as a way to model effects over time and generate results that were generalizable to the ever-enrolled Medicare population. That is, it consists of multiple years of data, and it includes persons who died and persons who became eligible for Medicare during the four-year study period. The MCBS provides no sampling weights appropriate for such a dataset, however. Merely using four years of cross-sectional weights would be inappropriate, and the MCBS provides longitudinal weights only for persons who were in the MCBS sample for the entire study period. The small panel dataset was created to take advantage of this set of weights. But the small panel must exclude deaths and newly eligibles in order to use the weights. Thus, its results are only generalizable to the continuously enrolled Medicare population. In the absence of appropriate sampling weights, the results for big panel dataset must be interpreted with caution.

A second set of limitations arises from the model specification and estimation methods used in the study. One concern is the possibility of omitted variable bias. The MCBS does not contain any variables that measure risk aversion or wealth, two factors that theoretically have a sizeable influence on a person's decision whether or not to join a Medicare HMO. While measures of the overall explanatory power of the HMO choice model were not reported by STATA for the panel estimations, the pseudo-R^2 for the cross-sectional estimation indicates that the model explains only 42 percent of the

variance. While it is unknown what the effective ceiling is for the predictive power of models of HMO choice (i.e., how much of the variation is truly stochastic), it is probably safe to say that there are some unobserved variables reflected in the error term that account for a portion of the unexplained variance. The possibility that the omission of these variables created bias in the coefficients on the included variables cannot be dismissed.

Omitted variable bias is always a concern in models of health care utilization as well. It is well-known that models of utilization generally can explain only a small proportion of the variance in health expenditures. Researchers have concluded that the highest achievable R^2 in such models is in the neighborhood of 0.15 to 0.20 (McCall and Wai, 1983; Welch, 1985; Newhouse et al., 1989; Manton and Stallard, 1992). Much of the unexplained variance in these models is probably truly random, due to unusual acute health events that cannot be predicted even if extensive information about a person's underlying health is obtained. However, some of the residual error is likely due to omitted variables that capture enduring but unmeasured aspects of a person's health and propensity to consume care.

By the standards of these studies, the utilization models in the present study had good predictive power. The R^2 statistics in the cross sectional estimations were 0.13 for probability of hospitalization, 0.20 for number of inpatient days, and 0.36 for the probability of a physician visit. The models captured several different dimensions of health status, including measures of chronicity. However, none of the variables measured illness severity very well. Moreover, there were no variables in the model

which measured a person's general propensity to seek medical care. Consequently, there is some possibility of omitted variable bias in the utilization equations.

A further limitation of the study is the sample size of the small panel dataset. The fact that so many of the righthand side variables achieved statistical significance in the big panel and even the cross-sectional estimations, but not in the small panel runs, suggests that there may be a power problem with the small panel. The number of HMO enrollees in this sample is small and not necessarily representative: only 696 out of the sample of 7,432 persons. Moreover, just 43 of these HMO enrollees had a hospitalization during the year. The sample size of the small panel caused a problem of perfect prediction with respect to two of the variables in the HMO choice equation, the dummy variables indicating whether or not there was a zero-premium HMO operating in the county and whether or not there was an HMO offering a prescription drug benefit. Consequently, these variables had to be dropped from the analysis. This was not a great loss since approximately 98 percent of observations had a "yes" value for these variables. Perfect prediction problems also necessitated collapsing some of the rural-urban continuum code categories. Again, this is not particularly problematic. A more subtle perfect prediction problem seemed to be operating with respect to the presence of the 16 chronic conditions variables in the physician visits model. Most likely, some combination of these variables perfectly predicted the outcome, causing the model to fail to converge. The problem was resolved by excluding some of the conditions variables from the small panel physician utilization model.

Additional concerns about the study results arise from the diagnostic findings regarding identification. While most of the tests indicated that the market penetration variables collectively constituted a valid set of exclusion restrictions, the Norton test results were mixed for some models. Thus, the evidence that identification was successfully achieved in the system of simultaneous equations is not unambiguous. It is, however, reasonably strong.

A final limitation to the analysis in this study derives from the practical difficulties associated with combining a number of different types of models: panel data analysis, simultaneous equations, a two-part model, and complex survey data. Neither STATA nor SAS has an algorithm that incorporates all of these elements of the analysis. In patching together commands to handle the various aspects of the models, some problems were encountered. For example, it was not possible to perform standard error corrections for the manual least squares in the inpatient days equation because XTGEE does not report the sum of squared errors, which is used in the correction factor. It also was not possible to perform NR^2 tests on the binary response models because XTPROBIT does not report R^2.

No analysis is perfect, and the more complicated it becomes, the less one should expect it to be. However, the reader is urged to consider the above limitations of the study when drawing inferences from its regression results.

9.2. Principal contributions of the study

This study contributes significantly to the existing body of research concerning selection bias and utilization in Medicare HMOs in a number of respects. The study makes three important findings:

1. There was only weak overall favorable selection into Medicare HMOs in the 1992-96 period.

2. The amount of favorable selection does not appear to be significantly related to the degree of Medicare HMO market penetration *at the county level*.

3. In the absence of uncontrolled-for favorable selection, Medicare HMOs significantly reduce the probability of hospitalization, as well as the number of hospital days used by those hospitalized. Medicare HMOs slightly increase, rather than decrease, the probability that a Medicare beneficiary will use physician services during the year.

These findings have considerable policy significance for HCFA. The findings concerning the effects of HMOs on utilization suggest that managed care is an effective cost-reducing strategy for the Medicare program. The finding that very little favorable selection persists in Medicare HMOs in the 1990s means that little or no additional risk adjustment may be needed in the AAPCC payment formula in order to prevent overpayment.

It should be noted that this study did not examine trends in disenrollment, which some researchers suggest contribute to the degree of favorable selection in Medicare HMOs. A highly-publicized study by Morgan and colleagues (1997) recently found that

Medicare beneficiaries who choose to disenroll from their HMOs are less healthy than beneficiaries in the general fee-for-service Medicare population, as measured by their post-disenrollment use of health services. The study's authors concluded that "the healthy go in and the sick go out" of Medicare HMOs. This study did not examine disenrollment patterns both because it was beyond the scope of the project and because in the particular dataset used, there were very few disenrollees. In the big panel of 38,185 beneficiaries, only 972 switched between HMO and fee-for-service plans. Other research indicates that in the general population, about one out of every five Medicare beneficiaries disenrolls from her HMO in a given year, with 75 percent of disenrollees returning to fee-for-service Medicare and 25 percent switching to another HMO (Brown et al. 1993; Langwell and Hadley 1990). If these individuals are disproportionately less healthy than those who stay in HMOs, then there may in fact be a "favorable retention" effect in Medicare HMOs today which was not modeled in the present study.

This study is the first to empirically examine the relationship between Medicare HMO market penetration and favorable selection. By linking MCBS data to market penetration data collected by HCFA, this study was able to examine a relationship that had previously only been the subject of speculation. The study failed to provide evidence of a county-level correlation between market penetration and favorable selection as postulated by the theory presented in Section 3. This is surprising given the strong temporal and regional correlation in the overall diminution of favorable selection in Medicare HMOs and the overall increase in Medicare HMO market penetration in the last fifteen years. One explanation for this result may be that market

penetration affects the extent of favorable selection on a national level, but not on a county level.

In addition to the results and policy implications of the study, several aspects of the study methodology constitute significant contributions in and of themselves. The use of 1992-96 data from the MCBS provides a needed update to earlier studies based on data from the 1980s. The use of simultaneous equations in this study also constitutes a significant innovation on existing studies. Prior studies of Medicare HMOs have not used simultaneous equations, but rather have employed one of three approaches: (1) single equation analyses of HMO choice or utilization, which do not take account of the endogeneity of HMO choice in a utilization equation; (2) sample selection models developed by Heckman (1979) and Lee (1978), which have been criticized for their instability and their assumption of a bivariate normal distribution for the error terms of the HMO choice and utilization equations (Hornbrook, Bennett, and Greenlick, 1989); and (3) a longitudinal approach proposed by Hornbrook, Bennett, and Greenlick (1989). The last approach involves first ascertaining whether selection bias exists by performing separate OLS estimations of utilization for person who switch from fee-for-service to HMOs and for fee-for-service stayers, and comparing the coefficients using a Chow (1960) test; and then testing the effects of HMO membership on utilization by running the utilization model twice on the switchers, once in the year prior to switching and once in the first HMO year, and comparing the coefficients. This approach suffers from several shortcomings, including bias due to the use of OLS to estimate utilization when there may be a large proportion of observations clustered at

zero; the possibility that utilization may be atypically high or low in the first year after joining an HMO; and the conceptual problems with measuring selection bias by trying to detect differences in the relationships between various health status measures and utilization, rather than by modeling plan choice itself. For example, it seems quite possible that fewer diabetics than non-diabetics might choose to switch to an HMO, but there might well be no difference in the coefficients on a "diagnosis of diabetes" variable in utilization equations for switchers and stayers in the year prior to the switch.

Researchers who have used the above methods have noted that a simultaneous system approach would be preferable to these approaches, but that the use of simultaneous equations was not possible due to a lack of variables to achieve identification of the utilization equation (Hornbrook, Bennett, and Greenlick, 1989). The simultaneous equations approach achieves the strong control for selection bias obtained by the Heckman/Lee model, but is more robust to distributional assumptions about the errors (Dubin and McFadden, 1984). The discrete factor model is particularly advantageous in this regard, as its semiparametric specification avoids the problems associated with a mistaken assumption of normality.

The present study achieves identification in the system of simultaneous equations through the exclusion of managed care market penetration variables from the utilization equations. No other study has attempted to resolve the identification problem in this way. Although the findings of one of the two specification tests used to test the validity of these exclusion restrictions were not uniformly favorable for all datasets and all equations, the overall picture emerging from the two tests is that the

market penetration variables were good instruments. They were jointly significant in the HMO equation, and generally not jointly significant when included in the utilization equations (with some exceptions, discussed earlier). Certainly there is a strong theoretical basis for the exclusion restriction: HMO market penetration seems quite likely to influence an individual's likelihood of belonging to an HMO, but does not have an obvious theoretical effect on the individual's health care utilization. While the non-uniform results of the Norton test of the identification restrictions should be noted, overall, the identification strategy appears sound, permitting this use of simultaneous equations to examine HMO membership and utilization in a new way.

The panel data approach of this study provides a useful contrast to previous studies that have examined selection bias and utilization in HMOs using cross-sectional data. Longitudinal data capture more information about individuals' health status and utilization than does a single measurement, and panel analysis is able to disaggregate the residual variation in utilization into within-person and between-person components.

In summary, this study produced important findings concerning favorable selection into Medicare HMOs and the effect of HMOs on utilization once selection effects are controlled for. Furthermore, the data, analytical methods, and theory used in this study represent significant innovations over previous studies. The successes and pitfalls of this attempt to model the complex relationship between health status, HMO choice, and subsequent health care utilization should assist future researchers in formulating modeling strategies for these and other endogenous variables. More

importantly, the empirical findings of the study may help policymakers determine the future of the AAPCC payment formula and the Medicare risk program itself.

APPENDIX A: PROGRAMMING NOTES

This Appendix discusses problems encountered in the programming of the models in STATA and model specification decisions made as a result of those problems. In its original formulation, the HMO choice equation included two additional variables, a binary indicator of whether there was a Medicare HMO offering a prescription drug benefit in the respondent's county and a binary indicator of whether there was a zero-premium Medicare HMO in the county. These variables had to be dropped from the model because each of them perfectly predicted the outcome (HMO choice). Specifically, there were no respondents who were HMO members but who lived in a county that did not have a zero-premium plan. There also were no respondents who were HMO members but who lived in a county that did not have a plan offering a drug benefit. The prevalence of zero-premium and drug benefit plans was simply too high to allow sufficient variation in these variables. While the perfect prediction problem was only observed for the small panel dataset, the variables were dropped from the model for all datasets in order to preserve the consistency of the model specification.

A perfect prediction problem was also encountered with the rural-urban continuum codes. The counts for some of the categories were very small. To eliminate the perfect prediction problem, six of the categories were collapsed into just three. The two categories "urbanized nonmetro" counties ("adjacent to MSA" and "not adjacent to

MSA") were combined, as were the adjacent and non-adjacent "less urbanized nonmetro" counties and the adjacent and non-adjacent "thinly populated nonmetro" counties. Again, while the perfect prediction problem was only present in the small panel dataset, the model respecification was applied to all three datasets in the interest of consistency.

In the course of running the HMO equation on the cross-sectional dataset, it was determined that there were no respondents missing data on the Medigap variable in that dataset. Consequently, the missingness indicator variable for Medigap was dropped from the model for that dataset only. There were observations missing Medigap data in the big panel and small panel datasets (581 and 78 observations, respectively), so the missingness indicator was retained in the models for those datasets. It was not thought necessary to maintain consistency across datasets on this point because the variable dropped was not substantively meaningful; it was purely a control variable, and there was nothing to control for in the cross-sectional dataset.

Modeling the 16 chronic conditions variables presented a particular problem. The original specification of the HMO equation, as well as all three utilization equations, included all 16 chronic conditions indicators plus a variable for total number of chronic conditions (out of a possible 17). It was later decided that it was undesirable to include both aggregate and individual measures of chronic conditions because the aggregate measure was almost a perfect linear combination of the individual condition variables. The individual condition variables were thought preferable to the aggregate measure because of the theoretical possibility that the coefficients on the individual

condition variables might have different signs, and the effects might cancel each other out if aggregated.

In order to determine whether all 16 individual condition variables belonged in the model, the unrestricted model was run on all three datasets, the significance levels of the individual variables were examined, and a Wald test was performed. Unfortunately, it was difficult to draw conclusions from the Wald test because test results differed between the datasets. For the big panel dataset, three of the variables were significant at the 5 percent level: history of arthritis, history of cancer, and history of stroke. The Wald test indicated that the 16 variables were jointly significant ($\chi^2(16)=37.13$; $p=0.002$). For the small panel dataset, only one of the variables was individually significant at the 5 percent level: history of rheumatoid arthritis. The Wald test found the 16 variables not to be jointly significant ($\chi^2(16)=22.44$; $p=0.1294$). For the cross-sectional dataset, only the arthritis variable was individually significant, and the Wald test found the variables not jointly significant ($\chi^2(16) = 22.73$, $p=0.1211$). The differences in these results could be due to the much larger sample size of the big panel dataset, or to the fact that the big panel dataset includes persons who died, or to the fact that the big panel analysis was unweighted.

Ultimately a decision was made to include only the three condition variables that were individually significant in the big panel analysis. The Wald tests for the small panel and cross-sectional datasets, which indicated the 16 condition variables were not jointly significant, are likely to be more reliable than the Wald test for the big panel since the standard errors in the big panel were not adjusted by sampling weights.

However, there was a strong indication in the big panel run that these three condition variables were significant predictors. Since it would be a more serious error to omit relevant variables than to include irrelevant variables (the former leading to biased coefficients and the latter causing only a lack of precision), these three variables were included in the HMO model for all datasets. Unfortunately, it was not possible to test this model specification using an NR^2 test because XTPROBIT does not report an R^2 statistic.

The condition variables also caused a problem in the physician utilization equation. Initial runs of the models on the small panel dataset revealed that the physician utilization model would not converge when both the aggregate chronic illness variable and all 16 chronic conditions variables were included in the model. After investigating possible problems of scaling and multicollinearity in the model, it was concluded that the lack of convergence must be due to a problem of perfect prediction arising from some linear combination of variables. The problem was unique to the small panel dataset.

Several options were explored for solving the problem. The model was run without the "total number of conditions" variable but including all 16 individual conditions variable; it would not converge. Several runs were attempted using different subsets of the 16 variables but excluding the "total number of conditions variable," but none of these combinations led to convergence. It was possible to run the model successfully including only the "total number of conditions" variable. This is not an ideal measure of chronic illness, however, since its continuous nature assumes that each

successive chronic illness would add equally to an individual's probability of having a physician visit, which is not necessarily true. In the end, the following modelling strategy was employed: Only the three individual conditions variables that were significant in the HMO equation (arthritis, cancer, and stroke) were included in the physician visit model for the small panel dataset. Convergence was successfully achieved for this model. Although it made for inconsistent models across datasets, all 16 of the chronic conditions variables were retained in the physician utilization equations for the big panel and cross-sectional datasets. It was important to include these variables where possible (i.e., where there was no convergence problem) because of the high theoretical likelihood that they would be statistically significant predictors of physician utilization.

An additional programming note relates to the use of Wald tests in the specification tests run on the models. As is described in greater depth in Section 6.3.2., three diagnostic tests were conducted: two tests of identifying restrictions proposed by Norton (oral communication) and Norton & Staiger (1994), and a test of exogeneity described by Pindyck and Rubinfeld (1981). Both the Norton and the Norton & Staiger tests were originally formulated around a likelihood ratio test. It is, however, inappropriate to use likelihood ratio tests where there is clustering or weighting in the dataset. The "likelihood" reported in regressions of such data is not a true likelihood, as it accounts for neither the clustering nor the weighting. Consequently, for this analysis, the likelihood ratio tests in these diagnostics were replaced by Wald tests.

Finally, it should be noted that no adjustments were made to any of the standard errors to account for the design effect of the MCBS's complex survey design. In order to determine whether the computational effort associated with making this adjustment was justified, the significance levels of the unadjusted, Huber-corrected standard errors from the regression output for all equations were examined. The idea of this examination was that if many of the significance levels were borderline (i.e., between 0.02 and 0.05), then adjusting for the design effect might push many variables into insignificance. This would indicate that the design effect adjustment ought to be made.

In fact, the examination revealed few instances of borderline significance levels. Moreover, most of the variables found to have borderline significance levels were control variables rather than variables of great theoretical import to the model. Primarily they were the census region and rural/urban residence variables. For the big panel dataset, there were two control variables with p-values between 0.02 and 0.05 in the HMO equation, one in the probability of hospitalization equation, and five each in the inpatient days equation and the physician visits equation. For the small panel dataset, there was one borderline control variable in the HMO equation, three in the probability of hospitalization equation, one in the inpatient days equation, and three in the physician visits equation. For the cross-sectional dataset, there were three control variables and one market penetration interaction term in the HMO equation that were of borderline significance. Two control variables were borderline in the probability of hospitalization equation, three were borderline in the inpatient days equation, and three were borderline in the physician visits equation. Importantly, in the inpatient days

equation, the endogenous variable, HMO membership, was also borderline ($p=0.047$). It is fair to say that this HMO variable would become statistically insignificant if the design effect adjustment were performed. However, this is only true for the cross-sectional dataset. Based on the results of this investigation as a whole, it was decided that there would be no significant gains associated with computing design effect-adjusted standard errors.

Turning now to the utilization equations, a significant problem was encountered when programming manual two-stage least squares for the continuous (inpatient days) equation. It was not possible to perform the correction to the second-stage standard errors to account for the fact that predicted, rather than actual, values of the endogenous variable are used in the second stage regression. It was discovered that neither the XTGEE nor the XTREG commands report the sum of squared errors, which is a necessary component of the standard error correction factor. Statisticians at the STATA Corporation were consulted for assistance in obtaining the correction factor through alternate means, but none could recommend any strategy. Consequently, the standard errors reported for the inpatient days 2SLS model are uncorrected. The standard errors in the discrete factor model results, of course, do not have this problem. Standard error corrections are not needed in the two-stage probit estimations; Monte Carlo experiments show that there are no significant gains associated with such adjustments (Guilkey, Mroz and Taylor, 1992).

During the course of the modeling, a question arose as to how to estimate a two-part model as part of an instrumental variables estimation. When the second equation is

run only on the conditional sample (in this case, persons with at least one hospitalization), should the predicted values from the first equation come from a regression including the entire sample, or only the conditional sample? As the answer to this question was not readily apparent, it was initially decided to do it both ways. However, it proved to be impossible to generate predicted values from the HMO equation based on the conditional sample only. The reason is that when the non-hospitalized persons were dropped from the sample, there were very few HMO members left. In the small panel dataset, there were only 44 HMO observations over all four years of data. A perfect prediction problem arose, and crosstabs revealed there to be perfect prediction on 7 different variables. As a result, the predicted values for the two-part model derive from a first-equation regression on the entire sample.

Another problem that arose in the course of running the models was that for the cross-sectional dataset only, the nursing home residence variable produced a problem of perfect prediction in the physician visit model. There were no persons in the sample who lived in a nursing home and who had a physician visit during 1996. This problem caused STATA to drop 873 observations for this model. Consequently, the model was rerun without the nursing home variable, and those results are reported herein. It should be noted that the nursing home variable was statistically significant in the physician visit models for both of the panel datasets; consequently, it is possible that its omission from the cross-sectional physician visit model resulted in some bias in the other coefficients.

Finally, as with the HMO equation, diagnostic tests performed on the utilization equations were based on Wald tests rather than likelihood ratio tests. Additionally, it was again not possible to do NR^2 tests on the binary response models because XTPROBIT does not report R^2. An NR^2 test was performed on the continuous (inpatient days) model, for which an R^2 can be obtained if one uses the XTREG command. However, the results of this test suggested that XTREG was not reporting the proper R^2. The results were not at all close to the p-value of a Wald test performed on the same model specification, creating suspicion about the validity of the NR^2 test.

WORKS CITED

Aday, L.A. and Andersen, R.M. "A Framework for the Study of Access to Medical Care." *Health Services Research*, Vol. 9 (1974), pp. 208-220.

Aday, L.A. and Andersen, R.M. *Access to Medical Care*. Ann Arbor: Health Administration Press, 1975.

Aday, L.A., Andersen, R.M., and Fleming, G.V. *Health Care in the U.S.: Equitable for Whom?* Beverly Hills: Sage Publications, 1980.

Adler, G.S. "A Profile of the Medicare Current Beneficiary Survey." *Health Care Financing Review*, Vol. 15 (1994), pp. 153-163.

Adler, G.S. "Medicare Beneficiaries Rate Their Medical Care: New Data from the MCBS." *Health Care Financing Review*, Vol. 16 (1995), pp. 175-187.

Alkire, A.A. and Stolz, S.W. "The Employer's View of Managed Health Care: From a Passive to an Aggressive Role." In P.R. Kongstvedt, ed., *The Managed Health Care Handbook*. Second edition. Gaithersburg, MD: Aspen Publishers, Inc., 1993, 255-264.

American Association of Health Plans. "Number of People Enrolled in HMOs, 1976-96." *AAHP Online web site*. 1996.

Andersen, R.M. *A Behavioral Model of Families' Use of Health Services*. Chicago: Center for Health Administration Studies, 1968.

Andersen, R.M. and Newman, J. "Societal and Individual Determinants of Medical Care Utilization in the United States." *Milbank Memorial Fund Quarterly*, Vol. 51 (1973), pp. 95-124.

Anderson, G. and Knickman, J.R. "Pattern of Expenditures Among High Utilizers of Medical Care Services." *Medical Care*, Vol. 22 (February 1984), pp. 143-149.

Arrow, K.J. *Essays in the Theory of Risk Bearing*. Chicago: Markham, 1971.

Aston, G. "Medicare HMO Exodus Leaves Physicians and Patients in Pinch." *American Medical News* (October 19, 1998).

Bashshur, R.L. and Metzner, C.A. "Patterns of Social Differentiation Between Community Health Association and Blue Cross-Blue Shield." *Inquiry*, Vol. 4 (1967), pp. 23-44.

Bashshur, R.L. and Metzner, C.A. "Vulnerability to Risk and Awareness of Dual Choice of Health Insurance Plan." *Health Services Research*, Vol. 5 (1970), pp. 106-113.

Bates, E.W. and Connors, K.A. "Assessing Process of Care Under Capitated and Fee-For-Service Medicare." *Health Care Financing Review*, Annual Supplement (1987), pp. 57-68.

Beebe, J.C. "Medicare Reimbursement and Regression to the Mean." *Health Care Financing Review*, Vol. 9 (Spring 1988), pp. 9-22.

Berki, S.E. and Ashcraft, L.F. "HMO Enrollment: Who Joins What and Why: A Review of the Literature." *Milbank Memorial Fund Quarterly*, Vol. 58 (1980), pp. 558-632.

Berki, S.E., Ashcraft, M.L.F., Penchansky, R. and Fortus, R.S. "Enrollment Choice in a Multi-HMO Setting: The Roles of Healthy Risk, Financial Vulnerability, and Access to Care." *Medical Care*, Vol. 15 (1977), pp. 95-114.

Bice, T.W. "Risk Vulnerability and Enrollment in a Prepaid Group Practice." *Medical Care*, Vol. 13 (1975), pp. 698-703.

Bollen, K.A., Guilkey, D.K., and Mroz, T.A. "Binary Outcomes and Endogenous Explanatory Variables: Tests and Solutions with an Application to the Demand for Contraceptive Use in Tunisia." *Demography*, Vol. 32 (February 1995), pp. 111-131.

Borch, K. *The Economics of Uncertainty*. Princeton, N.J.: Princeton University Press, 1968.

Breusch, T. and Pagan, A. "A Simple Test for Heteroscedasticity and Random Coefficient Variation." *Econometrica*, Vol. 47 (1979), pp. 1287-1294.

Brown, R. *Biased Selection in the Medicare Competition Demonstration*. Contract No. 500-83-0047. Prepared for the Health Care Financing Administration. Washington: Mathematica Policy Research, Inc., February 1987.

Brown, R. *Biased Selection in the Medicare Competition Demonstrations*. Princeton, NJ: Mathematica Policy Research, Inc., 1988.

Brown, R.S., Clement, D.G., Hill, J.W., Retchin, S.M., and Bergeron, J.W. "Do Health Maintenance Organizations Work for Medicare?" *Health Care Financing Review*, Vol. 15 (Fall 1993), pp. 7-23.

Brown, R.S. and Langwell, K. "Enrollment Patterns in Medicare HMOs: Implications for Access to Care." *Advances in Health Economics and Health Services Research*, Vol. 9 (1988), pp. 69-96.

Broyles, R.W. and Rosko, M.D. "The Demand for Health Insurance and Health Care: A Review of the Empirical Literature." *Medical Care Review*, Vol. 45 (Fall 1988), pp. 291-338.

Buchanan, J.L. and Cretin, S. "Risk Selection of Families Electing HMO Membership." *Medical Care* , Vol. 24 (January 1986), pp. 39-51.

Buchanan, J.L., Leibowitz, A., and Keesey, J. "Medicaid Health Maintenance Organizations: Can They Reduce Program Spending?" *Medical Care*, Vol. 34 (1996), pp. 249-263.

Burner, S.T. and D.R. Waldo. "National Health Expenditure Projections, 1994-2005." *Health Care Financing Review*, Vol. 16 (Summer 1995), pp. 221-242.

Calore, K. and Lim, J. *Results of the National Medical Expenditure Survey Household Survey Medicare Record Component Pretest*. Report submitted to the National Center for Health Services Research and Health Care Technology Assessment, Contract No. 282-86-0013. Waltham, MA: Health Economics Research, 1989.

Cannell, C.F. and Fowler, F.J. "A Comparison of a Self-Enumerative Procedure and a Personal Interview: A Validity Study." *Public Opinion Quarterly*, Vol. 27 (1963), pp. 250-264.

Cardin, B. "Rep. Cardin Reintroduces Bill to Aid Seniors Affected by HMO Pullout of Medicare." Government Press Release by Federal Document Clearing House, Inc., February 9, 1999.

Centers for Disease Control and Prevention. "Prevention and Managed Care: Opportunities for Managed Care Organizations, Purchasers of Health Care, and Public Agencies." *Morbidity and Mortality Weekly Report*, Vol. 44 (November 17, 1995), pp. 10-12.

Chow, G.C. "Tests of Equality Between Sets of Coefficients in Two Linear Regressions." *Econometrica*, Vol. 28 (July 1960), pp. 591-605.

Clement, D.G., Retchin, S.M., Brown, R.S., and Stegall, M.H. "Access and Outcomes of Elderly Patients Enrolled in Managed Care." *JAMA*, Vol. 271 (1994), pp. 1487-1492.

Cohen, J. and Cohen, P. *Applied Multiple Regression/Correlation Analysis for the Behavioral Sciences*. Hillsdale, NJ: Lawrence Erlbaum Associates, Publishers, 1983.

Collins, K.S. and Sandman, D. "Physicians' Experiences with Managed Care." *Bulletin of the New York Academy of Medicine*, Vol. 73 (Winter Supplement 1996), pp. 490-495.

Congressional Budget Office. *Catastrophic Medical Expenses: Patterns in the Non-Elderly, Non-Poor Population*. Washington: CBO, 1982.

Congressional Budget Office. *CBO March 1996 Baseline: Medicare*. Washington: CBO, March 1996.

Congressional Budget Office. *Cost Estimate for HR 2485 (Medicare Preservation Act of 1995)*. Washington: CBO, 1995.

Cunningham, F.C. and Williamson, J.W. "How Does the Quality of Health Care in HMO's Compare to That in Other Settings? An Analytic Literature Review: 1958 to 1979." *Group Health Journal*, Vol. 1 (1980), pp. 4-25.

Davidson, B.N. "Designing Health Insurance Information for the Medicare Beneficiary: A Policy Synthesis." *Health Services Research*, Vol. 23 (December 1988), pp. 685-720.

Davies, A.R. "Selection of Prepaid or Fee-for-Service Care By Medicaid Families: Examining the Health-Risk Hypothesis." *Advances in Health Economics and Health Services Research*, Vol. 10 (1989), pp. 183-198.

DesHarnais, S.I. "Enrollment in and Disenrollment From Health Maintenance Organizations by Medicaid Recipients." *Health Care Financing Review*, Vol. 6 (Spring 1985), pp. 39-50.

Dowd, B. and Feldman, R. "Biased Selection in Twin Cities Health Plans." *Advances in Health Economics and Health Services Research*, Vol. 6 (1985), pp. 253-271.

Dowd, B.E., Johnson, A.N., and Madson, R.A. "Inpatient Length of Stay in Twin Cities Health Plans." *Medical Care*, Vol. 24 (1986), pp. 694-710.

Dowd, B., Moscovice, I., Feldman, R., Finch, M., Wisner, C., and Hillson, S. "Health Plan Choice in the Twin Cities Medicare Market." *Medical Care*, Vol. 32 (1994), pp. 1019-1039.

Duan, N., Manning, W.G., Morris, C.N., and Newhouse, J.P. "A Comparison of Alternative Models for the Demand for Medical Care." *Journal of Business and Economic Statistics*, Vol. 1 (1983), pp. 115-126.

Duan, N., Manning, W.G., Morris, C.N., and Newhouse, J.P. "Choosing Between the Sample-Selection Model and the Multi-Part Model." *Journal of Business and Economic Statistics*, Vol. 2 (1984), pp. 283-289.

Dubin, J.A. and McFadden, D.L. "An Econometric Analysis of Residential Electric Appliance Holdings and Consumption." *Econometrica*, Vol. 52 (1984), pp. 345-362.

Eggers, P. "Risk Differential Between Medicare Beneficiaries Enrolled and Not Enrolled in an HMO." *Health Care Financing Review*, Vol. 1 (Winter 1980), pp. 91-99.

Eggers, P.W. and Prihoda, R. "Pre-Enrollment Reimbursement Patterns of Medicare Beneficiaries Enrolled in 'At-Risk' HMOs." *Health Care Financing Review*, Vol. 4 (September 1982), pp. 55-72.

Ellis, R.P. and Ash, A. *Refining the Diagnostic Cost Group Model: A Proposed Modification to the AAPCC for HMO Reimbursement*. Grant No. 18-C-98526/1-03-HCFA. University Health Policy Consortium, Brandeis/Boston University, Waltham, Massachusetts.

Ellis, R.P. and McGuire, T.G. "Setting Capitation Payments in Markets for Health Services." *Health Care Financing Review*, Vol. 8 (Summer 1987), pp. 55-64.

Ellis, R.P., Pope, G.C., Iezzoni, L.I., Ayanian, J.Z., Bates, D.W., Burstin, H. and Ash, A.S. "Diagnosis-Based Risk Adjustment for Medicare Capitation Payments." *Health Care Financing Review*, Vol. 17 (Spring 1996), pp. 101-128.

Emmons, D.W. and Simon, C.J. "Recent Trends in Managed Care." In M.L. Gonzalez, ed., *Socioeconomic Characteristics of Medical Practice 1994*. Chicago: American Medical Association, 1994.

Emmons, D.W. and Simon, C.J. "Managed Care: Evolving Contractual Arrangements." In M.L. Gonzalez, ed., *Socioeconomic Characteristics of Medical Practice 1996*. Chicago: American Medical Association, 1996.

Emmons, D.W. and Wozniak, G.D. "Physicians' Contractual Arrangements with Managed Care Organizations." In M.L. Gonzalez, ed., *Socioeconomic Characteristics of Medical Practice 1997*. Chicago: American Medical Association, 1997.

Eppig, F.J., Jr. and Edwards, B. "Computer Matching of Medicare Current Beneficiary Survey Data With Medicare Claims." In R. Warnecke, ed., *Health Survey Research Methods Conference Proceedings*, DHHS Publication No. (PHS) 96-1013. Hyattsville, MD: United States Department of Health and Human Services, April 1996.

Epstein, A.M. and Cumella, E.J. "Capitation Payment: Using Predictors of Medical Utilization to Adjust Rates." *Health Care Financing Review*, Vol. 10 (Fall 1988), pp. 51-69.

Farley, P.J. and Monheit, A.C. "Selectivity in the Demand for Health Insurance and Health Care." *Advances in Health Economics and Health Services Research*, Vol. 6 (1985), pp. 231-248.

Feldman, R., Dowd, B., Finch, M., and Cassou, S. *Employer-Based Health Insurance*. DHHS Publication No. (PHS) 89-3434. Rockville, MD: United States Department of Health and Human Services, June 1989.

Freund, D.A. and Hurley, R.E. "Medicaid Managed Care: Contribution to Issues of Health Reform." *Annual Review of Public Health*, Vol. 16 (1995), pp. 473-495.

Freund, D.A., Rossiter, L.F., Fox, P.D., Meyer, J.A., Hurley, R.E., Carey, T.S., and Paul, J.E. "Evaluation of the Medicaid Competition Demonstrations." *Health Care Financing Review*, Vol. 11 (Winter 1989), pp. 81-97.

Friedman, B. "Risk Aversion and the Consumer Choice of Health Insurance Option." *Review of Economics and Statistics*, Vol. 56 (1948), pp. 209-214.

Friedman, M. and Savage, L.J. "The Utility Analysis of Choices Involving Risk." *Journal of Political Economy*, Vol. 56 (1948), pp. 279-304.

Garnick, D. and Short, T. "Utilization of Hospital Inpatient Services by Elderly Americans." *National Center for Health Services Research and Health Care Technology Assessment*. DHHS Pub. No. (PHS) 85-3351. Rockville: Public Health Service, 1985.

Garfinkel, S.A., Riley, G.F., and Iannacchione, V.G., "High-Cost Users of Medical Care." *Health Care Financing Review*, Vol. 9 (Summer 1988), pp. 41-52.

Garfinkel, S.A., Schlenger, W.E., McLeroy, K.R., Bryan, F.A., York, B.J.G., Dunteman, G.H., and Friedlob, A.S. "Choice of Payment Plan in the Medicare Capitation Demonstration." *Medical Care*, Vol. 24 (July 1986), pp. 628-640.

Gillick, M.R. "The Impact of Health Maintenance Organizations on Geriatric Care." *Annals of Internal Medicine*, Vol. 106 (January 1987), pp. 139-143.

Godfrey, L. "Testing for Multiplicative Heteroscedasticity." *Journal of Econometrics*, Vol. 8 (1978), pp. 227-236.

Goldman, Dana P. "Managed Care as a Public Cost-Containment Mechanism." *RAND Journal of Economics*, Vol. 26 (Summer 1995), pp. 277-295.

Gonzalez, M.L. *Physician Marketplace Statistics 1996*. Chicago: American Medical Association, 1997.

Grazier, K.L., Richardson, W.C., Martin, D.P., and Diehr, P. "Factors Affecting Choice of Health Plans." *Health Services Research*, Vol. 20 (1986), pp. 659-682.

Green, M. "Medicare HMOs at Risk: Behind the Pullout Wave." *Best's Review—Life-Health Insurance Edition*. A.M. Best Company, Inc., 1998.

Greene, W.H. *Econometric Analysis*. 2nd edition. Englewood Cliffs, NJ: Prentice Hall, 1993.

Greenlick, M.R., Lamb, S.J., Carpenter, T.M., Fischer, T.S., Marks, S.D., and Cooper, W.J. "Kaiser-Permanente's Medicare Plus Project: A Successful Medicare Prospective Payment Demonstration." *Health Care Financing Review*, Vol. 4 (June 1983), pp. 85-97.

Gruenberg, L., Kaganova, J., and Hornbrook, M.C. "Improving the AAPCC With Health-Status Measures from the MCBS." *Health Care Financing Review*, Vol. 17 (Spring 1996), pp. 59-75.

Gruenberg, L., Tompkins, C., and Porell, F. "The Health Status and Utilization Patterns of the Elderly: Implications for Setting Medicare Payments to HMOs." *Advances in Health Economics and Health Services Research*, Vol. 10 (1989), pp. 41-73.

Guilkey, D.K., Mroz, T.A., and Taylor, L. "Estimation and Testing in Simultaneous Equations Models with Discrete Outcomes Using Cross Section Data." Unpublished manuscript. 1992.

Gujarati, D.N. *Basic Econometrics*. 3rd edition. New York: McGraw-Hill, Inc., 1995.

Harvey, A. "Estimating Regression Models with Multiplicative Heteroscedasticity." *Econometrica*, Vol. 44 (1976), pp. 461-465.

Hausman, J.A. "Specification Tests in Econometrics." *Econometrica*, Vol. 46 (November 1976), pp. 1251-1271.

Hausman, J.A. "Specification and Estimation of Simultaneous Equations Models." In Z.Griliches and M. Intriligator, eds., *Handbook of Econometrics*. Amsterdam: North-Holland, 1983.

Health Care Financing Administration. "1992 Statistical Supplement." *Health Care Financing Review* (1992).

Health Care Financing Administration. "1995 Statistical Supplement." *Health Care Financing Review* (1995).

Health Care Financing Administration. "1996 Statistical Supplement." *Health Care Financing Review* (1996a).

Health Care Financing Administration, "Medicaid Managed Care Enrollment Report." (June 30, 1996b).

Heckman, J.J. "Sample Selection Bias as a Specification Error." *Econometrica*, Vol. 45 (1979), pp. 153-161.

Hellinger, F.J. "Selection Bias in Health Maintenance Organizations: Analysis of Recent Evidence." *Health Care Financing Review*, Vol. 9 (Winter 1987), pp. 55-63.

Hellinger, F.J. "Selection Bias in HMOs and PPOs: A Review of the Evidence." *Inquiry*, Vol. 32 (Summer 1995), pp. 135-142.

Herzog, A.R. and Dielman, L. "Age Differences in Response Accuracy for Factual Survey Questions." *Journal of Gerontology*, Vol. 40 (May 1985), pp. 350-357.

Hill, J.W. and Brown, R.S. *Biased Selection in the TEFRA HMO/CMP Program.* Princeton, NJ: Mathematica Policy Research, Inc., 1990.

Hill, J.W. and Brown, R.S. *Health Status, Financial Barriers, and the Decision to Enroll in Medicare Risk Plans*. Princeton, NJ: Mathematica Policy Research, Inc., 1992.

Hoerger, T.J. and Howard, L.Z. "Search Behavior and Choice of Physician in the Market for Prenatal Care." *Medical Care*, Vol. 33 (1995), pp. 332-349.

Hornbrook, M.C., Bennett, M.D., and Greenlick, M.R. "Adjusting the AAPCC for Selectivity and Selection Bias Under Medicare Risk Contracts." *Advances in Health Economics and Health Services Research*, Vol. 10 (1989), pp. 111-149.

Hoy, E.W., Curtis, R.E., and Rice, T. "Change and Growth in Managed Care." *Health Affairs* (Winter 1991), pp. 18-36.

Idler, E. "Self-Assessed Health and Mortality: A Review of Studies." In S. Maes, H. Leventhal and M. Johnston, eds., *International Review of Health Psychology*. New York: John Wiley and Sons, 1992.

Iglehart, J.K. "Changing With the Times: The Views of Bruce C. Vladeck." *Health Affairs*, Vol. 16 (May/June 1997), pp. 58-71.

Iglehart, J.K. "Health Policy Report: Medicare Turns to HMO's." *New England Journal of Medicine*, Vol. 312 (1985), pp. 132-136.

Iglehart, J.K. "Medicaid and Managed Care." *New England Journal of Medicine*, Vol. 332 (June 22, 1995), pp. 1727-1731.

Jackson-Beeck, M. and Kleinman, J.H. "Evidence for Self-Selection Among Health Maintenance Organization Employees." *JAMA* , Vol. 250 (November 25, 1983), pp. 2826-2829.

Jacob, J.A. "United HMOs Leave 86 Medicare Markets." *American Medical News* (October 19, 1998).

Jobe, J.B., White, A.A., Kelley, C.L., Mingay, D.J., Sanchez, M.J., and Loftus, E.F. "Recall Strategies and Memory for Health-Care Visits." *Milbank Quarterly*, Vol. 68 (1990), pp. 171-189.

Juba, D.A., Lave, J.R., and Shaddy, J. "An Analysis of the Choice of Health Benefits Plans." *Inquiry*, Vol. 17 (Spring 1980), pp. 62-71.

Kahneman, D. "New Challenged to the Rationality Assumption." *Journal of Institutional and Theoretical Economics*, Vol. 150 (1994), pp. 18-36.

Kahneman, D. and Tversky, A. "Prospect Theory: An Analysis of Decision Under Risk." *Econometrica*, Vol. 47 (1979), pp. 263-291.

Kane, R.L. "Health Care Reform and the Care of Older Adults." *Journal of the American Geriatric Society*, Vol. 43 (1995), pp. 702-706.

Kasper, J.D., Riley, G.F., McCombs, J.S., and Stevenson, M.A. "Beneficiary Selection, Use, and Charges in Two Medicare Capitation Demonstrations." *Health Care Financing Review*, Vol. 10 (Fall 1988), pp. 37-49.

Lairson, D. R. and Herd, J.A. "The Role of Health Practices, Health Status, and Prior Health Care Claims in HMO Selection Bias." *Inquiry*, Vol. 24 (1987), pp. 276-284.

Lamphere, J.A., Neuman, P., Langwell, K., and Sherman, D. "The Surge in Medicare Managed Care: An Update." *Health Affairs*, Vol 16 (May/June 1997), pp. 127-133.

Langwell, K.M. and Hadley, J.P. "Evaluation of the Medicare Competition Demonstrations." *Health Care Financing Review*, Vol. 11 (Winter 1989), pp. 65-79.

Langwell, K.M. and Hadley, J.P. "Insights from the Medicare HMO Demonstrations." *Health Affairs* (Spring 1990), pp. 74-84.

Lee, L.F. "Unionism and Wage Rates: A Simultaneous Equations Model With Qualitative and Limited Dependent Variables." *International Economic Review*, Vol. 19 (June 1978), pp. 415-433.

Lee, E.S., Forthafer, R.N. and Lorimer, R.J. *Analyzing Complex Survey Data*. Sage University Paper Series on Quantitative Applications in the Social Sciences, No. 07-071. 1989.

Leopold, J. and Langwell, K. "Review of the Evidence on Demand for Health Care." *Report of the National Commission on the Cost of Medical Care*. Vol. 3. Chicago: American Medical Association, 1978.

Lichtenstein, R., Thomas, J.W., Adams-Watson, J., Lepkowski, J., and Simone, B. "Selection Bias in TEFRA At-Risk HMOs." *Medical Care*, Vol. 29 (April 1991), pp. 318-331.

Loftus, E.F., Smith, K.D., Klinger, M.R., and Fiedler, J. "Memory and Mismemory for Health Events." In J.M. Tanur, ed.,*Questions About Questions: Inquiries into the Cognitive Bases of Surveys*. New York: Russell Sage Foundation, 1992.

Lubitz, J., Beebe, J., and Riley, G. "Improving the Medicare HMO Payment Formula to Deal With Biased Selection." *Advances in Health Economics and Health Services Research*, Vol. 6 (1985), pp. 101-122.

Luce, R.D. and Suppes, P. "Preference, Utility, and Subjective Probability." In R.D. Luce, R. Bush, and E. Galanter, eds., *Handbook of Mathematical Psychology*. 3rd edition. New York: John Wiley, 1965.

Luft, H. "Assessing the Evidence on HMO Performance." In J.B. McKinlay, ed., *Health Maintenance Organizations*. Cambridge, MA: MIT Press, 1981.

Luft, H. *Health Maintenance Organizations: Dimensions of Performance*. New York: John Wiley & Sons, 1981.

Luft, H.S. "Medicare and Managed Care." *Annual Review of Public Health*, Vol. 19 (1998), pp. 459-475.

Lurie, N., Moscovice, I.S., Finch, M., Christianson, J.B., and Popkin, M.K. "Does Capitation Affect the Health of the Chronically Mentally Ill? Results from a Randomized Trial." *JAMA*, Vol. 267 (1992), pp. 3300-3304.

Maddala, G.S. *Limited Dependent and Qualitative Variables in Econometrics*. Cambridge: Cambridge University Press, 1983.

Manning, J. "PrimeCare Gold Cuts 25% of Its Providers." *Milwaukee Journal Sentinel*, February 10, 1999.

Manning, W.G., Leibowitz, A., Goldberg, G.A., Rogers, W.H., and Newhouse, J.P. "A Controlled Trial of the Effect of a Prepaid Group Practice on Use of Services." *New England Journal of Medicine*, Vol. 310 (June 7, 1984), pp. 1505-1510.

Manton, K.G. and Stallard, E. "Analysis of Underwriting Factors for AAPCC." *Health Care Financing Review*, Vol. 14 (Fall 1992), pp. 117-132.

Marquis, K. "Record Checks for Sample Surveys." T. Jabine, M. Straf, J. Tanur, and R. Tourangeau, eds., *Cognitive Aspects of Survey Methodology: Building a Bridge Between Disciplines*. Washington: National Academy Press, 1984.

McCall, N. and Wai, H.S. "An Analysis of the Use of Medicare Services by the Continuously Enrolled Aged." *Medical Care*, Vol. 21 (June 1983), pp. 567-585.

McCombs, J., Kasper, J., and Riley, G. "The Use and Cost of Services for Medicare Beneficiaries and Capitated Systems: A Multivariate Analysis." Paper presented at the American Public Health Association Annual Meeting. Las Vegas, October 1986.

McFadden, D. "Conditional Logit Analysis of Qualitative Choice Behavior." In P. Zarembka, ed., *Frontiers in Econometrics*. New York: Academic Press, 1973.

Mechanic, D. "Consumer Choice Among Health Insurance Options." *Health Affairs*, Vol. 8 (Spring 1989), pp. 138-148.

Mechanic, D., Ettel, T., and Davis, D. "Choosing Among Health Insurance Options: A Study of New Employees." *Inquiry*, Vol. 27 (Spring 1990), pp. 14-23.

Merrill, J., Jackson, C., and Reuter, J. "Factors That Affect the HMO Enrollment Decision: A Tale of Two Cities." *Inquiry*, Vol. 22 (1985), pp. 388-395.

(Anonymous.) "Year in Review 1998." *Modern Healthcare* (December 21, 1998), p. 26.

Morgan, R.O., Virnig, B.A., DeVito, C.A., and Persily, N.A. "The Medicare-HMO Revolving Door—The Healthy Go In and the Sick Go Out." *New England Journal of Medicine*, Vol. 337 (July 1997), pp. 169-175.

Morrison, E.M. and H.S. Luft. "Health Maintenance Organization Environments in the 1980s and Beyond." *Health Care Financing Review*, Vol. 12 (Fall 1990), pp. 81-90.

Moustafa, A.T., Hopkins, C.E., and Klein, B. "Determinants of Choice and Change in Health Insurance Plans." *Medical Care*, Vol. 9 (1971), pp. 32-41.

Nakamura, A. and Nakamura, M. "On the Relationship Among Several Specification Error Tests Presented by Durbin, Wu, and Hausman." *Econometrica*, Vol. 49 (November 1981), pp. 1583-1588.

National Center for Health Statistics, "The Longitudinal Study of Aging 1984-1990." *Vital and Health Statistics*, Series 1, No. 28. (July 1992).

Neipp, J. and Zeckhauser, R. "Persistence in the Choice of Health Plans." *Advances in Health Economics and Health Services Research*, Vol. 6 (1985), pp. 47-72.

Nelson, L., Rossiter, L.F., and Adamache, K. *Final Report on the Analysis of Aggregate Use and Cost Data from Medicare HMO's*. Contract No. 500-83-0047. Prepared for the Health Care Financing Administration. Washington: Mathematica Policy Research, Inc., June 1986.

Newhouse, J.P., Buntin, M.B., and Chapman, J.D. "Risk Adjustment and Medicare: Taking a Closer Look." *Health Affairs*, Vol. 16 (September/October 1997).

Newhouse, J., Manning, W., Keeler, E., and Sloss, E. "Adjusting Capitation Rates Using Objective Health Measures and Prior Utilization." *Health Care Financing Review*, Vol. 10 (Spring 1989), pp. 41-54.

Newhouse, J.P., Manning, W.G., Morris, C.N., Orr, L.L., Duan, N., Keeler, E.B., Leibowitz, A., Marquis, K.H., Marquis, M.S., Phelps, C.E., and Brook, R.H. "Some Interim Results From a Controlled Trial of Cost Sharing in Health Insurance." *New England Journal of Medicine*, Vol. 305 (1981), pp. 1501-1507.

Norton, E.C. and Staiger, D.O. "How Hospital Ownership Affects Access to Care for the Uninsured." *RAND Journal of Economics*, Vol. 25 (Spring 1994), pp. 171-185.

Oberlander, J.B. "Managed Care and Medicare Reform." *Journal of Health Politics, Policy, and Law*, Vol. 22 (April 1997), pp. 595-631.

Phelps, C.E. "Demand for Reimbursement Insurance." In R.N. Rosett, ed., *The Role of Health Insurance in the Health Services Sector*. New York: National Bureau of Economic Research, 1976.

Physician Payment Review Commission. *Estimated Payment Rates for MedicarePlus Plans*. Washington: Government Printing Office, 1995.

Pindyck, R. and Rubinfeld, D. *Econometric Models and Economic Forecasts*. New York: McGraw-Hill, 1981.

Porell, F.W. and Wallack, S.S. "Medicare Risk Contracting: Determinants of Market Entry." *Health Care Financing Review*, Vol. 12 (Winter 1990), pp. 75-85.

Price Waterhouse. *Is There Biased Selection in Medicare HMOs?* Washington, D.C.: Price Waterhouse, March 1996.

Public Health Service. *Reporting of Hospitalization in the Health Interview Survey*. DHEW-HSM Publication No. 73-1261. Washington: Government Printing Office, July 1965.

Retchin, S.M. and Brown, B. "Elderly Patients With Congestive Heart Failure Under Prepaid Care." *American Journal of Medicine*, Vol. 90 (1991), pp. 236-242.

Retchin, S.M., Brown, R.S., Yeh, S.J., Chu, D., and Moreno, L. "Outcomes of Stroke Patients in Medicare Fee for Service and Managed Care." *JAMA*, Vol. 278 (July 9, 1997), pp. 119-124.

Retchin, S.M., Clement, D.G., Rossiter, L.F., Brown, B., Brown, R., and Nelson, L. "How the Elderly Fare in HMOs: Outcomes from the Medicare Competition Demonstrations." *Health Services Research*, Vol. 27 (December 1992), pp. 651-669.

Reuben, C., Pickle, L., Madans, J., Rothwell, S., Kleinman, J., and Feldman, J. "Correlates of Accurate Respondent Reporting of Hospital Stays." Abstract for Presentation at the 1991 Meetings of the Society of Epidemiologic Research. 1991.

Rice, T. "Can Markets Give Us the Health System We Want?" *Journal of Health Politics, Policy and Law*, Vol. 22 (April 1997), pp. 383-426.

Riley, G., Lubitz, J., Prihoda, R., and Rabey, E. "The Use and Costs of Medicare Services by Cause of Death." *Inquiry*, Vol. 24 (1987), pp. 233-244.

Riley, G., Lubitz, J. and Rabey, E. "Enrollee Health Status Under Medicare Risk Contracts: An Analysis of Mortality Rates." *Health Services Research*, Vol. 26 (June 1991), pp. 137-63.

Riley, G., Rabey, E. and Kasper, J. "Biased Selection and Regression Toward the Mean in Three Medicare HMO Demonstrations: A Survival Analysis of Enrollees and Disenrollees." *Medical Care*, Vol. 27 (April 1989), pp. 337-347.

Riley, G., Tudor, C., Chiang, Y., and Ingber, M. "Health Status of Medicare Enrollees in HMOs and Fee-for-Service in 1994." *Health Care Financing Review*, Vol. 17 (Summer 1996), pp. 65-76.

Roemer, M.I., Hetherington, R.W., Hopkins, C.E., Gerst, A.E., Parsons, E., and Long, D.M. *Health Insurance Effects: Services, Expenditures, and Attitudes Under Three Types of Plans*. Bureau of Public Health Economics Research Series No. 16. Ann Arbor: University of Michigan School of Public Health, 1972.

Rodgers, W.L. and Herzog, A.R. "Interviewing Older Adults: The Accuracy of Factual Information." *Journal of Gerontology*, Vol. 42 (July 1987), pp. 387-394.

Roos, N.P. and Shapiro, E. "The Manitoba Longitudinal Study on Aging: Preliminary Findings on Health Care Utilization by the Elderly." *Medical Care*, Vol. 19 (1981), pp. 644-657.

Rosenbach, M.L., Harrow, B.S., and Hurdle, S. "Physician Participation in Alternative Health Plans." *Health Care Financing Review* (Summer 1988), pp. 63-79.

Rossiter, L.F., Friedlob, A., and Langwell, K. "Exploring Benefits of Risk-Based Contracting Under Medicare." *Healthcare Financial Management*, Vol. 39 (May 1985), pp. 42-58.

Rossiter, L.F., Langwell, K.M., Brown, R., Adamache, K.W., and Nelson, L. "Medicare's Expanded Choices Program: Issues and Evidence from the HMO Experience." *Advances in Health Economics and Health Services Research*, Vol. 10 (1989), pp. 3-40.

Russell, S. "HMOs Are Sacking Seniors. Medicare Patients Angry, Fearful." *San Francisco Chronicle* (November 9, 1998), p. A1.

Schauffler, H.H., Howland, J., and Cobb, J. "Using Chronic Disease Risk Factors to Adjust Medicare Capitation Payments." *Health Care Financing Review*, Vol. 14 (Fall 1992), pp. 79-90.

Schneider, E.L. "Cutting the Costs of Aging." *National Academy of Sciences: Issues in Science and Technology*, Vol. 7 (1991), pp. 47-49.

Schuttinga, J.A., Falik, M., and Steinwald, B. "Health Plan Selection in the Federal Employees Health Benefits Program." *Journal of Health Politics, Policy and Law*, Vol. 10 (Spring 1985), pp. 119-139.

Scitovsky, A., McCall, N., and Benham, L. "Factors Affecting the Choice Between Two Prepaid Plans." *Medical Care*, Vol. 16 (August 1978), pp. 660-681.

Sherbourne, C.D. and Meredith, L.S. "Quality of Self-Report Data: A Comparison of Older and Younger Chronically Ill Patients." *Journal of Gerontology*, Vol. 47 (July 1992), pp. S204-S211.

Simonson, I. "The Effect of Purchase Quantity and Timin on Variety-Seeking Behavior." *Journal of Marketing Research*, Vol. 27 (1990), pp. 150-162.

Sisk, J.E., Gorman, S.A., Reisinger, A.L., Glied, S.A., DuMouchel, W.H., and Hynes, M.M. "Evaluation of Medicaid Managed Care: Satisfaction, Access, and Use." *JAMA*, Vol. 276 (July 3, 1996), pp. 50-55.

Siu, A.L., Brook, R.H., and Rubenstein, L.Z. "Medicare Capitation and Quality of Care for the Frail Elderly." *Health Care Financing Review*, Annual Supplement (1986), pp. 57-63.

Sofaer, S. and Hurwicz, M. "When Medical Group and HMO Part Company: Disenrollment Decisions in Medicare HMOs." *Medical Care*, Vol. 31 (1993), pp. 808-821.

Spitz, B. "When A Solution Is Not A Solution: Medicaid and Health Maintenance Organizations." *Journal of Health Politics, Policy and Law* (Winter 1979).

Spitz, B. and Abramson, J. "Competition, Capitation, and Case Management: Barriers to Strategic Reform." *Milbank Quarterly*, Vol. 65 (1987), pp. 348-370.

Stearns, S.C., Kovar, M.G., Hayes, K., and Koch, G.G. "Estimates of National Hospital Use from Administrative Data and Personal Interviews." *Journal of Official Statistics*, Vol. 12 (1996), pp. 47-61.

Stern, R.S., Juhn, P.I., Gertler, P.J., and Epstein, A.M. "A Comparison of Length of Stay and Costs for Health Maintenance Organization and Fee-for-Service Patients." *Archives of Internal Medicine*, Vol. 149 (May 1989), pp. 1185-1188.

Strumwasser, I., Paranjpe, N.V., Ronis, D.L., McGinnis, J., Kee, D.W., and Hall, H.L. "The Triple Option Choice: Self-Selection Bias in Traditional Coverage, HMOs, and PPOs." *Inquiry*, Vol. 26 (Winter 1989), pp. 432-441.

Taylor, A.K., Beauregard, K.M., and Vistnes, J.P. "Who Belongs to HMOs: A Comparison of Fee-for-Service Versus HMO Enrollees." *Medical Care Research and Review*, Vol. 52 (September 1995), pp. 389-408.

Tessler, R. and Mechanic, D. "Factors Affecting the Choice Between Prepaid Group Practice and Alternative Insurance Programs." *Milbank Memorial Fund Quarterly*, Vol. 53 (Spring 1975), pp. 149-172.

Thomas, J.W., Berki, S.E., Lichtenstein, R., and Wyszewianski, L. *A Health Status Measure for Adjusting the HMO Capitation Rates of Medicare Beneficiaries.* Final Report of Grant 18-P-98179/5-01 with the Health Care Financing Administration. 1984.

Tobin, J. "Estimation of Relationships for Limited Dependent Variables." *Econometrica*, Vol. 26 (1958), pp. 24-36.

Trieger, S., Galblum, T., and Riley, G. *HMO's: Issues and Alternatives for Medicare and Medicaid.* HCFA Pub. No. 03107. Washington: Government Printing Office, April 1981.

United States Department of Health and Human Services, Health Care Financing Administration. *Medicare Current Beneficiary Survey, Access to Care,*

Calendar Year 1992: [United States] [User Documentation]. 2nd ICPSR release. Baltimore, MD: United States Department of Health and Human Services, Health Care Financing Administration [producer], 1996(a). Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 1997.

United States Department of Health and Human Services, Health Care Financing Administration, Office of the Actuary, Office of National Health Statistics. *Medicare Current Beneficiary Survey CY 1992 Cost and Use Public Use File Documentation*. Baltimore, MD: United States Department of Health and Human Services, Health Care Financing Administration, May 1996(b).

United States Department of Health and Human Services, Health Resources and Services Administration, Bureau of Health Professions. *Bureau of Health Professions Area Resource File, 1940-1994: [United States]* [User Documentation]. Rockville, MD: United States Department of Health and Human Services, Office of Data Analysis and Management [producer], 1995. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 1995.

United States General Accounting Office. *Medicare Issues Raised by Florida Health Maintenance Organizations. Report to Congress*. GAO/HRD-86-97. Washington: Government Printing Office, 1986.

United States House of Representatives, Committee on Ways and Means, *Medicare and Health Care Chartbook*. Washington: Government Printing Office, 1997.

United States Senate, Special Committee on Aging. *Aging America: Trends and Projections*. Washington: Government Printing Office, 1989.

van Vliet, R., and van de Ven, W. "Capitation Payments Based on Prior Hospitalizations." *Econometrics and Health Economics*, Vol. 2 (1993), pp. 177-188.

Wagner, E.R. "Types of Managed Care Organizations." In P.R. Kongstvedt, ed., *The Managed Health Care Handbook*. Second edition. Gaithersburg, MD: Aspen Publishers, Inc., 1993.

Waldo, D.R., Sonnefeld, S.T., McKusick, D.R., and Arnett, R.H. "Health Expenditures by Age Group, 1977 and 1987." *Health Care Financing Review*, Vol. 10 (Summer 1989), pp. 111-120.

Wan, T.T.H., "The Behavioral Model of Health Care Utilization by Older People." In M.G. Ory and K.B. Bond, eds., *Aging and Health Care: Social Science and Policy Perspectives*. London: Routledge Press, 1989.

Ware, J.E., Bayliss, M.S., Rogers, W.H., Kosinski, M., and Tarlov, A.R. "Differences in 4-Year Health Outcomes for Elderly an Poor, Chronically Ill Patients Treated in HMO and Fee-for-Service Systems: Results from the Medical Outcomes Study." *JAMA*, Vol. 276 (1996), pp. 1039-1047.

Ware, J.E., Rogers, W.H., Davies, A.R., Goldberg, G.A., Brook, R.H., Keeler, E.B., Sherbourne, C.D., Camp, P., and Newhouse, J.P. "Comparison of Health Outcomes at a Health Maintenance Organisation With Those of Fee-for-Service Care." *Lancet* , Vol. 1 (May 3, 1986), pp. 1017-1022.

Weil, P. "Comparative Costs to the Medicare Program of Seven Prepaid Group Practice Plans." *Milbank Memorial Fund Quarterly*, Vol. 54 (1976), pp. 339-365.

Weisbrod, B.A. *Competition in the Health Care Sector: Past, Present, and Future.* Washington: Bureau of Economics, Federal Trade Commission, 1978.

Weissman, J.S., Levin. K., Chasan-Taber, S., Massagli, M.P., Seage, G.R., and Scampini, L. "The Validity of Self-Reported Health-Care Utilization by AIDS Patients." *AIDS*, Vol. 10 (June 1996), pp. 775-783.

Welch, W.P. "Defining Geographic Areas to Adjust Payments to Physicians, Hospitals, and HMOs." *Inquiry*, Vol. 28 (Summer 1991), pp. 151-160.

Welch, W.P. "Growth in HMO Share of the Medicare Market, 1989-1994." *Health Affairs* , Vol. 15 (Fall 1996), pp. 210-214.

Welch, W.P. "Medicare Capitation Payments to HMOs in Light of Regression Toward the Mean in Medical Care Costs: Implications for Biased Selection in Health Maintenance Organizations." *Medical Care*, Vol. 23 (1985), pp. 1234-1241.

Welch, W.P. and Frank, R.G. "The Predictors of HMO Enrollee Populations: Results From a National Sample." *Inquiry*, Vol. 23 (Spring 1986), pp. 16-22.

Wentland, E.J. and Smith, K.W. *Survey Responses: An Evaluation of Their Validity.* San Diego: Academic Press, Inc., 1993.

Whitmore, R.W., Paul, J.E., Gibbs, D.A., and Beebe, J.C. "Using Health Indicators in Calculating the AAPCC." *Advances in Health Economics and Health Services Research*, Vol. 10 (1989), pp. 75-109.

Wiener, J. and Hanley, R. *Measuring the Activities of Daily Living Among the Elderly: A Guide to National Surveys*. Washington: Interagency Forum on Aging-Related Statistics, 1989.

Wilensky, G.R. and Rossiter, L.F. "Patient Self-Selection in HMOs." *Health Affairs*, Vol. 5 (Spring 1986), pp. 66-80.

Wilner, S., Schoenbaum, S.C., Monson, R.R., and Winickoff, R.N. "A Comparison of the Quality of Maternity Care Between a Health Maintenance Organization and Fee-for-Service Practices." *New England Journal of Medicine*, Vol. 304 (1981), pp. 784-787.

Wolinsky, F. "The Performance of Health Maintenance Organizations: An Analytic Review." In J.B. McKinlay, ed., *Health Maintenance Organizations*. Cambridge, MA: MIT Press, 1981.

Wolinsky, F.D., "Health Services Utilization Among Older Adults: Conceptual, Measurement, and Modeling Issues in Secondary Analysis." *Gerontologist*, Vol. 34 (1994), pp. 470-475.

Wolinsky, F.D. and Coe, R.M. "Physician and Hospital Utilization Among Noninstitutionalized Elderly Adjults: An Analysis of the Health Interview Survey." *Journal of Gerontology*, Vol. 39 (1984), pp. 334-341.

Wolinsky, F.D., Coe, R.M., Miller, D.K., Prendergast, J.M., Creel, M.J., and Chavez, M.N. "Health Services Utilization Among the Noninstitutionalized Elderly." *Journal of Health and Social Behavior*, Vol. 24 (1983), pp. 325-337.

Wolinsky, F.D. and Johnson, R.J. "The Use of Health Services by Older Adults." *Journal of Gerontology*, Vol. 46 (1991), pp. S345-S357.

Wolinsky, F.D., Mosely, R.R., and Coe, R.M. "A Cohort Analysis of the Use of Health Services by Elderly Americans." *Journal of Health and Social Behavior*, Vol. 27 (1986), pp. 209-219.

Woolridge, J.M. "Specification Testing and Quasi-Maximum-Likelihood Estimation." *Journal of Econometrics*, Vol. 48 (1991), pp. 29-56.

Yelin, E.H., Henke, C.J., Kramer, J.S., Nevitt, M.C., Shearn, M., and Epstein W.V. "A Comparison of the Treatment of Rheumatoid Arthritis in Health Maintenance Organizations and Fee-for-Service Practices." *New England Journal of Medicine*, Vol. 312 (1985), pp. 962-967.

Yelin, E.H., Shearn, M.A., and Epstein, W.V. "Health Outcomes for a Chronic Disease in Prepaid Group Practice and Fee-for-Service Settings." *Medical Care*, Vol 24 (1986), pp. 236-46.

Zarabozo, C. and LeMasurier, J.D. "Medicare and Managed Care." In P.R. Kongstvedt, ed., *The Managed Health Care Handbook*. Second edition. Gaithersburg, MD: Aspen Publishers, Inc., 1993, 321-344.

Zarabozo, C., Taylor, C., and Hicks, J. "Medicare Managed Care: Numbers and Trends." *Health Care Financing Review*, Vol. 17 (Spring 1996), pp. 243-261.

CMS LIBRARY
3 8095 00006059 6
C.1